PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE EIGHTH

SESSION 1914-15

PART II

SECTION OF EPIDEMIOLOGY AND STATE MEDICINE
SECTION OF THE HISTORY OF MEDICINE LARYNGOLOGICAL SECTION
MEDICAL SECTION NEUROLOGICAL SECTION
OBSTETRICAL AND GYNÆCOLOGICAL SECTION

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1915

LONDON :
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE EIGHTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1914-15

SECTION OF EPIDEMIOLOGY & STATE MEDICINE

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1915

Section of Epidemiology and State Medicine.

OFFICERS FOR THE SESSION 1914-15.

President—CHARLES J. MARTIN, M.B., F.R.S.

Vice-Presidents—

W. H. HAMER, M.D.
G. B. LONGSTAFF, M.D.
J. C. McVAIL, M.D.
Sir PATRICK MANSON, G.C.M.G., M.D., F.R.S.
Sir SHIRLEY F. MURPHY, F.R.C.S.
A. NEWSHOLME, C.B., M.D.
J. NIVEN, M.B.
Col. J. LANE NOTTER, M.D.
Sir ARTHUR WHITELEGGE, K.C.B., M.D.

Hon. Secretaries—

G. S. BUCHANAN, M.D.
WILLIAM BUTLER, M.B.

Hon. Secretary for Navy—Fleet-Surgeon P. W. BASSETT-SMITH, C.B., R.N.

Hon. Secretaries for Army—

Col. R. H. FIRTH, R.A.M.C., F.R.C.S.
Lieut.-Col. A. M. DAVIES, R.A.M.C.

Hon. Secretary for Scotland—A. K. CHALMERS, M.D.

Foreign and Colonial Hon. Secretaries—

Sir ARTHUR WHITELEGGE, K.C.B., M.D. (France).
R. H. SALTET, M.D. (Holland and Belgium).
S. MONCKTON COPEMAN, M.D., F.R.S. (Germany).
FRANK G. CLEMOW, M.D. (Russia).
ARTHUR SHADWELL, M.D. (Portugal).
Major F. A. SMITH, I.M.S., M.D. (Baluchistan).
ARMAND RUFFER, C.M.G., M.D. (Egypt and Red Sea).
Professor KITASATO (Japan).
R. BRUCE LOW, M.D. (South America).
Sir GEORGE TURNER, M.B. (Transvaal).
J. A. LOWSON, M.B. (China).
F. TIDSWELL, M.B. (New South Wales).
BOLTON G. CORNEY, I.S.O. (Polynesia).

Other Members of Council—

E. F. BASHFORD, M.D.
EDGAR L. COLLIS, M.B.
F. J. H. COUTTS, M.D.
F. G. CROOKSHANK, M.D.
R. DUDFIELD, M.B.
K. W. GOADBY.
E. W. GOODALL, M.D.
MAJOR GREENWOOD, jun.
Surgeon-General W. G. MACPHERSON, R.A.M.C., C.M.G., M.B.
CHARLES E. PAGET.
Sir RONALD ROSS, K.C.B., F.R.S.
J. E. SANDILANDS, M.D.
F. M. SANDWITH, M.D.
W. J. R. SIMPSON, C.M.G., M.D.
T. H. C. STEVENSON, M.D.
C. W. F. YOUNG, M.D.

Representative on Library Committee—E. W. GOODALL, M.D.

Representative on Editorial Committee—G. S. BUCHANAN, M.D.

SECTION OF EPIDEMIOLOGY AND STATE MEDICINE.

CONTENTS.

November 27, 1914.

PAGE

The Recent Epidemic of Small-pox in New South Wales: Its Diagnosis and Prevention. By W. G. Armstrong, M.B., Ch.M., D.P.H. 1

The Nature of the Recent Small-pox Epidemic in Australia: Microbiological Findings and Animal Inoculations. By J. Burton Cleland, M.D., Ch.M.Syd., and E. W. Ferguson, M.B., Ch.M.Syd. 19

February 26, 1915.

SPECIAL DISCUSSION ON THE EPIDEMIOLOGY OF CEREBROSPINAL MENINGITIS.

Opened by Sir William Osler, Bt., M.D., F.R.S. 41

The President (Dr. Charles J. Martin, F.R.S.) (p. 47)—Dr. Newsholme (p. 48)—Dr. Chalmers (p. 50)—Dr. Gardner Robb (p. 55)—Captain Ellis (p. 60), Dr. Newton (p. 62), Dr. Colebrook (p. 62), The President (p. 64).

March 5, 1915.

Special Discussion on the Epidemiology of Cerebrospinal Meningitis (*continued*):—

The President (Dr. Charles J. Martin, F.R.S.) 65

The Bacteriology of Epidemic Cerebrospinal Meningitis. By J. C. G. Ledingham 66

The Interpretation of Epidemiological Observations by the Light of Bacteriological Knowledge. By Joseph A. Arkwright, M.D. 69

Dr. W. H. Hamer (p. 77)—Professor Simpson (p. 78)—Dr. F. E. Batten (p. 79)—Dr. J. Robertson (p. 80)—Lieut.-Colonel Wilkinson (p. 81)—

309102

PAGE

Dr. W. E. Bullock (p. 82)—Dr. Hugh Thursfield (p. 82)—Dr. Mitford Atkinson (p. 82)—The President (Dr. Charles J. Martin, F.R.S.) (p. 83)—Captain John Dale (p. 84)—Dr. Tunnicliffe (p. 85)—Dr. Carnegie Dickson (p. 85)—Dr. d'Este Emery (p. 86)—Dr. G. S. Buchanan (p. 86)—Dr. A. Mearns Fraser (p. 87).

April 23, 1915.

Small-pox among Cotton Operatives. By H. E. Corbin, B.Sc., D.P.H. ... 89

Brief Note on Cotton as a Source of Small-pox Infection, as illustrated by an Outbreak of the Disease in Milnrow Urban District and the adjoining Borough of Rochdale. By Hugh A. Macewen, M.B. 100

The Occurrence of an Unusual Cough among Weavers of Cotton Cloth. By Edgar L. Collis, M.B. 108

June 4, 1915.

The Statistics of Anti-typhoid and Anti-cholera Inoculations, and the Interpretation of such Statistics in general. By Mr. Major Greenwood, jun., and Mr. G. Udny Yule 113

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

SOURCE

Section of Epidemiology and State Medicine.

November 27, 1914.

Dr. C. J. MARTIN, F.R.S., President of the Section, in the Chair.

The Recent Epidemic of Small-pox in New South Wales: Its Diagnosis and Prevention.

By W. G. ARMSTRONG, M.B., Ch.M., D.P.H.[1]

THE epidemic which is the subject of this paper has been of an exceedingly mild type. The fact that among over 1,000 cases only one death occurred (that of a parturient woman suffering from small-pox, who died of collapse two and a half hours after childbirth) constitute it indeed almost a unique record in the way of small-pox epidemics. In certain other directions the symptoms observed varied so widely from the classical picture presented by small-pox that at the beginning it was considered the disease was varicella. Unfortunately the earliest cases were exceptionally mild even when compared with the average of the whole epidemic, and the persons first affected were so slightly ill and the eruptions experienced by them so scanty, that no medical man was consulted by them, and subsequent inquiries have led to the conclusion that small-pox must have been present in Sydney for at least six weeks and affected an unknown number of persons before the Department of Public Health had any knowledge of the presence of an unusual form of eruptive disease in the State.

For permission to use the official data I am indebted to the Director General of Public Health for New South Wales (Dr. R. T. Paton), under whose direction the epidemic was controlled. I am also indebted to Dr. C. W. Reid, Chief Federal Quarantine Officer for New South Wales for permission to examine and use the records of the Quarantine Hospital.

On May 30, 1913, the owner and manager of a factory for the

[1] Senior Medical Officer of Public Health, New South Wales.

manufacture of underclothing, situated in Chalmers Street, Sydney, and employing over 200 hands, chiefly girls, reported that a number of his employees had during the months of April and May suffered from unusual rashes which appeared to be infectious. A medical officer of the Department of Public Health visited the factory the same day and learned that between April 20 and the end of May a number of young women employed in the factory had suffered with slight eruptions, for the most part on the face, but also extending to other parts of the body. In each case the appearance of the eruption had been preceded by an attack of so-called "influenza," in which the most prominent symptoms had been headache, vertigo, shivering, and general pains. In some of the cases there had been backache, and in a few there had also been some vomiting, usually slight. None of the girls affected had ever been vaccinated. The precedent illness in all the cases appeared to have been of a very mild type, and while some of the girls had stayed away from business for a few days, some had not considered it necessary to do so. Not more than two of the girls affected had consulted a medical practitioner, and in those cases they stated they had been told that there was nothing much the matter.

Further inquiries elicited that the first case in the factory had occurred on or about April 25, at which date a girl, E. D., aged 22, had been attacked by "influenza." Three days later she had developed a "pimply" rash on the face. She had been absent from work for a week and had returned to duty feeling well before the eruption appeared.

At first E. D. denied all knowledge of any possible source of infection for her illness, but later she stated to the manager of the factory, and subsequently to the writer of this paper, that she was very friendly with a young man, B. E., who had been a steward on the steamship "Zealandia," and who had arrived in Sydney on April 12, from Vancouver, British Columbia. She saw him at his mother's house on the day of his arrival and the following days, and he was then suffering from an eruption on his face.

The young man, B. E., was afterwards seen and cross-questioned. He was aged 23, and was said to have been vaccinated in infancy, but showed no scars of the operation. He was (on June 26) slightly marked on the forehead with six shallow, depressed cicatrices, and with several smaller ones on the cheeks. On questioning he gave the following information: He left Vancouver, British Columbia, in the "Zealandia" on March 19 and arrived in Sydney on April 12.

The ship touched at Victoria, B.C., Honolulu, Suva and Auckland, New Zealand, and B. E. went ashore at all ports. While ashore at Suva on April 4 he fell sick with severe headache, pains in the stomach and great weakness (no vomiting), and was confined to his bunk for two and a half days after leaving Suva, but did not see the ship's surgeon. His illness therefore appeared sixteen days after leaving Vancouver. The day after leaving Auckland—i.e., on April 9—his face "broke out" in pimples. The eruption was scanty and was confined to the face, forehead and back of the neck, and before it appeared he felt quite well. He considered that the rash was a crop of small boils and it did not entirely disappear from his face until some days after his arrival in Sydney. No one else on board so far as he knew suffered from a similar condition. He admitted that he knew the girl E. D. well, and saw her several times after his arrival in Sydney. The subsequent history of this man, B. E., is best given at this point, though somewhat out of logical sequence;—

On July 3 he was vaccinated; seven days later there was no sign of a normal "take." The site of vaccination presented three small "mulberry" marks slightly raised above the skin without any signs of vesiculation and without any surrounding areola. There was no swelling or gland enlargement. However, on July 16, thirteen days after vaccination, on re-examination he showed three large pocks with slight brawny induration of the arm. The pocks were not exactly similar in appearance to ordinary vaccination lesions, but resembled them to a certain extent and contained a yellowish lymph. There had been considerable delay in their appearance compared with the usual results of vaccination. As far as could be ascertained no other person than the girl E. D. who came into contact with B. E. suffered from small-pox.

On examination of the employees of the Chalmers Street factory, it was found that nearly all the persons said to have been affected showed small stains, dried scales, and in a few instances very shallow depressed cicatrices upon some portion of their bodies. In one or two instances these had been fairly numerous, but in several cases the total number observed upon any one person did not amount to twenty, while in some there were fewer still. One girl, for instance, had three such marks on her chest, and one on one arm. Another had twelve on the face, three on the right arm, and one on the left arm. In some instances girls who had suffered from precisely similar symptoms ("influenza") to the others had recovered completely and the symptoms had not been followed by any eruption.

On being questioned, the girls described the rash as having appeared as "pimples" and having come out in crops, which continued to erupt from day to day over a period of from three or four days to a week. Most of the girls appeared to have been rather more affected on the face than elsewhere, but this was not always the case, and in some instances the face had escaped altogether.

Every girl affected by the eruption stated that after the attack of "influenza" and before the rash appeared she felt quite well and continued so, and had returned to work either immediately, or as soon as she was "fit to be seen."

The factory itself was in a new building, excellently planned in regard to lighting, ventilation, and sanitation, and maintained in an exceedingly cleanly and orderly manner. Most of the employees were engaged in making under-garments with the aid of sewing machines run by power.

On the same date one of the girls (G. H.) employed at the Chalmers Street factory was admitted into the Coast Hospital, Little Bay, suffering from a copious rash on her face, body, and limbs. She had been seen by two medical practitioners, who diagnosed her illness as a severe form of chicken-pox. On May 31 she was seen by medical officers of the Health Department in consultation with the Medical Superintendent. Her temperature was then normal and she did not appear to be ill. She also was unvaccinated. The eruption in her case was more plentiful than in any of the other cases which had been examined, but the absence of any serious symptoms of constitutional illness, when weighed with the symptoms observed in the other cases that had occurred at the factory, led to the formation of the opinion by the medical officers that they were dealing with a form of chicken-pox.

The occurrence of a relatively severe case in a patient in Sydney Hospital first caused serious doubts as to the nature of what had been previously regarded as varicella. Even this case, though in some of its features it conformed to the classical type of small-pox, was of so mild a nature in its constitutional reactions (though the patient was an unvaccinated girl) as to make the diagnosis a matter of some doubt. This case was reported to the Department of Public Health on June 18, and on June 18 and 19 six persons who had never been vaccinated, but who were known to have suffered from the disease hitherto regarded as varicella, were persuaded to allow themselves to be vaccinated under control conditions. By control conditions I mean that in each case other unvaccinated persons were vaccinated at the same time from the

same tube of lymph. The results of this experiment were very definite. None of the six persons who had been attacked by the illness reacted to the vaccination, while all the controls developed perfect vaccination lesions within the usual time.

These results were considered very suspicious and the investigation was continued, and by July 1 the officers of the Department of Public Health were able to submit such a body of evidence that persons who had suffered from the suspected disorder did not react to vaccination as to warrant the formal proclamation of the presence of small-pox in New South Wales.

The subjects of inquiry indicated by the title of this discussion are the diagnosis and prevention of small-pox, and the indication afforded by recent events in New Zealand and New South Wales more particularly limits the scope of the inquiry to the form of small-pox recently experienced in the localities named. As a preliminary then it may be permitted briefly to consider the question as to the nature of the epidemics with which New Zealand and New South Wales have been afflicted. Speaking for the profession in Sydney, there is an entire unanimity among medical men who had any experience of cases in regarding the epidemic there as one of true but mild small-pox, and the evidence afforded by one New Zealand case which invaded New South Wales is in favour of the two epidemics being essentially the same in type.

The evidence as to the identity of the New South Wales epidemic with true small-pox may be briefly summarised as follows: The symptomatology is essentially the same, only differing in intensity of the manifestations. The period of incubation was the same. Vaccination was found to protect against the Sydney disease and an attack of the disorder was protective on the other hand against vaccination. The liability to abortion among pregnant women and the infection of the fœtus which are so characteristic of classical small-pox, were also observed in the epidemic in Sydney. The more severe cases were accompanied by the typical variolous odour and followed by the depressed scarring or pitting of the skin which is the usual sequela of classical small-pox. So far as one can speak at the present time, the pitting promises to be permanent.

All these characteristics except the last named were entirely absent in the numerous cases of chicken-pox which were under observation in Sydney during the small-pox epidemic. Chicken-pox was unusually prevalent and unusually severe in Sydney during the year 1913. Over

3,000 cases of chicken-pox were seen and diagnosed by the staff of the Health Department during the epidemic of small-pox, and with very few exceptions the experienced staff found no difficulty in differentiating between the two diseases. Many instances occurred of chicken-pox among recently vaccinated persons, and on the other hand in many cases persons recovering or but lately recovered from chicken-pox were successfully vaccinated during the months of the epidemic. These observations were in marked contrast to the facts in relation to the cases of small-pox.

The theory that the disease observed in Sydney was a hybrid is unscientific and will not bear examination for a moment. The behaviour of the two diseases towards vaccination entirely negatives any such supposition.

Small-pox, like all other diseases, has varied in intensity in different outbreaks. Mild outbreaks have been observed from very early times. Sydenham states that "small-pox has its peculiar kinds which take one form during one series of years, and another during another." Jenner himself records an outbreak in which the virulence was surprisingly low. He says: "It was of so mild a nature that a fatal instance was hardly ever heard of, and consequently so little dreaded by the lower orders that they scrupled not to hold the same intercourse with each other as if no infectious disease had been present among them," and Van Swieten in 1759 describes an outbreak which was "so mild that secondary fever is not manifested and constantly is wanting, convalescence coming on on the eighth day of the eruption."

To come to more recent times, during 1896 a very mild type of small-pox began to prevail in the South of the United States and gradually spread over the whole country and into Canada. In 1902 it invaded the island of Trinidad and 5,154 cases were reported with but twenty-eight deaths. In 1911 Chapin records that 3,294 cases occurred in North Carolina without a single death. The same writer states that it is highly probable that the mild form of small-pox observed in England during 1901 and 1902 was introduced from America. Chapin further states that while this atypical form of small-pox was spreading in America the classical and more severe form appeared from time to time in various places. Both forms always appeared to "breed true," but he adds that "health officers and clinicians all over the country who have had an extensive experience of the old type of the disease as well as with the other form are convinced of their identity. The reasons are the same period of incubation, the characteristic though mild prodromal symptoms, the development

of the eruption at the normal time, the distribution of the eruption, and its perfectly characteristic appearance in most cases. The crucial test of the identity of the two forms is, however, to be found in their immunity relations. . . . Persons who have had small-pox or who have been successfully vaccinated are at least as immune to the mild as to the severe type. It is also found that persons who have had the mild type are equally immune to vaccinia." [1]

After having swept through the United States this mild type of small-pox invaded Canada, in parts of which it still persists. Official records show that it actually prevailed in British Columbia during the early months of 1913, and therefore at the time of the departure of the steamer "Zealandia" for Sydney with the steward B. E. on board.

The diagnosis of the mild type of small-pox, like that of the severer form, depends upon (1) the distribution of the focal rash, (2) the characters of the focal lesion, and (3) the onset of the illness and its relation to the appearance of the rash.

The Distribution of the Rash.

The modern method of diagnosing small-pox gives precedence to the distribution of the rash above all other evidences. In former times it was the accepted teaching to regard as more important the form or anatomical characters of the individual lesion. To quote Ricketts, "The evidence from position there can be little doubt is in most cases more intrinsically valuable than the evidence from character; it is certainly the more easily observed and the more generally dependable." The broad features of the distribution of the rash in the classical picture of small-pox are that the rash prefers the upper half of the body to the lower, that it is a rash of the face and arms rather than of the trunk and legs; that it affects the distal rather than the proximal ends of the limbs, the back of the trunk rather than the front, the extensor surfaces rather than the flexor. It prefers, in short, those surfaces which are most liable to irritation.

These broad features applied generally in the Sydney epidemic and were on the whole relied on by us as the most important evidence presented to us in diagnosing a case. In individual instances anomalies of distribution did occur and were sometimes marked, but these anomalies could generally be explained and allowed for if one did not forget the effects which precedent irritation of the skin always has upon the rash of small-pox. In three cases among pregnant women, for instance,

[1] *Journ. Infect. Dis.*, Chicago, 1913, xiii, p. 180.

which I saw myself, the rash appeared first upon the abdomen and continued unusually plentiful there throughout the period of efflorescence. This was due to the friction of the clothing upon the distended and prominent surface of a part of the body which usually escapes very lightly from the focal rash of small-pox.

In many of the cases of small-pox seen in the New South Wales epidemic the numerical severity of the rash was so low that all the pocks on the whole surface of the body numbered less than twenty, and even in some cases as few as three or four. In such cases it is impossible to expect the distribution of the rash to comply with any law. Nevertheless, when taken broadly, the rash of mild small-pox as a whole does follow the laws of distribution observed by the classical type of the disease and with surprising accuracy. In a doubtful case one has sometimes adopted the expedient suggested by Wanklyn of plotting out a plan of the rash upon one of those diagrams of the human figure which are sold by medical booksellers for recording physical signs, and this was always very helpful in obtaining a clear impression of the distribution of the rash and a considerable aid in diagnosis. Usually in the milder cases of the epidemic the rash first appeared as a pimple on the nose or forehead, and in the majority of cases the rash on the face continued throughout efflorescence to exhibit greater numerical intensity than on any other part of the body. Nevertheless, instances were not wanting throughout the epidemic in which the face escaped altogether, but these were rare. Next to the face, the eruption affected the extensor surfaces of the forearms, then the back (especially between the shoulders), next the buttocks and thighs. The chest, and still more the abdomen, was almost invariably relatively and often entirely free from rash. This, of course, constituted a very useful diagnostic stigma in differentiating the disease from chicken-pox. The scalp and the male and female genital organs (the penis and vulva) were very frequently invaded. In all the more severe cases the palms of the hands and soles of the feet were attacked, but in the milder cases they often escaped altogether. In the more severe forms of the disease the formation of the pocks beneath the thick skin of the soles was often attended by a great deal of pain, and these pocks usually became inspissated and formed brown, seed-like bodies which had to be dug out with a knife on convalescence.

Although the eruption was usually scanty and sparse in the type of small-pox seen in Sydney it was not always so. Instances in which the rash was confluent on the face and arms occurred, and not rarely a

semi-confluent rash in those situations was observed. In one patient in whom the pocks were counted they numbered over 4,000 upon the whole surface of the body, of which 362 were upon the face. In another instance there were over 900 counted upon the face alone. In the latter case nearly all the pocks were small and badly developed. These cases were exceptional however, and the average number of lesions upon the face in the majority of instances ranged from about twelve to, say, thirty or more.

The Individual Lesion.

The focal lesion in the Sydney epidemic manifested great variations both in its anatomical characteristics and in its development. In a small minority of the severer cases it approximated in both directions very closely to that of the classical type of small-pox. In such cases the rash appeared in a single crop, the lesion passed through the regular stages of papule, vesicle and pustule, and the whole evolution from the first appearance of the papule to the beginning of incrustation occupied about eight days. The vesicles in these rare cases were circular, flat-topped, or umbilicated, greyish in colour, and when pricked or incised the contents were not wholly discharged, indicating loculation of the vesicle.

In the overwhelming majority of our cases, however, the regular stages of development of the lesion were obliterated or nearly so, and the whole period of evolution was much shortened. Often the first sign of the rash was an outcrop of pimples on the nose or forehead, which acquired pustular heads within a few hours, and sometimes the changes were so rapid that the lesions seemed almost to make their first appearance as pustules. They exhibited a strong tendency to appear in crops, which continued to erupt from day to day throughout the whole period of efflorescence, until the earlier lesions had dried up or become scabbed over. As a consequence, lesions were generally present at the same time on the same part of the body in various stages of development—a state of matters which is not generally distinctive of small-pox.

Another direction in which the eruption in our epidemic was aberrant was in the large proportion of small and abortive lesions. In most cases of the classical variety of small-pox a minority of the lesions are small and abortive, but in the New South Wales epidemic such abortive lesions were often in the immense majority.

Most of the variations from the classical type observed in our epidemic appeared to depend very largely upon the superficial character

of the lesions. This characteristic showed itself best in the absence of pitting of the skin after recovery which was seen in most of the cases, and in the shallowness of the little ulcers which were seen when a pustule was accidentally ruptured (about 10 per cent. of the patients, however, did show pitting). When the lesions were fairly large their superficial character was also shown by the appearance of the margin of the pustule. In chicken-pox the pustule or vesicle often appears to be lying upon the surface of the skin. In ordinary small-pox it juts through the skin, throwing the surrounding skin up in a slope round the base of the pustule. In this form of small-pox the appearance presented by a full-sized lesion is half-way between the two. It is less superficial than in chicken-pox, and more so than in ordinary small-pox. The same fact accounts for the sinuous, jagged outline and oval shape of the lesions which were often seen in the Sydney epidemic, especially in those lesions situated on the trunk.

It was the relative or complete absence of the eruptive fever which accounts for the exceeding mildness of the epidemic. Even in cases which passed as severe the maturation of the pustules was accompanied with very little elevation of temperature. In the case of the girl E. B., who was admitted into the Sydney Hospital, and whose illness in many respects was more typical of the ordinary form of small-pox than almost any other case seen in the epidemic, she never had a temperature to speak of after the rash appeared. She was admitted into the hospital on June 12, and the rash first appeared on June 15. Previous to the appearance of the rash she had a toxæmic fever running very near 105° F., but after the rash had once appeared her temperature only once touched 101° F., and that was on June 20, the day after her removal to the Coast Hospital. On June 21 it fell to 99° F., and on June 22 to normal, from which it never rose again. All through the stage of pustulation she stated that she felt perfectly well, and complained of not getting enough meat to eat.

In most of the cases sent to quarantine the temperature continued normal throughout the eruptive stage, and the patients did not exhibit any symptoms of that profound illness which usually accompanies pustulation in the unvaccinated.

The Invasion Period.

The mode of onset of the illness—the third member of the diagnostic trinity enumerated—would be of greater value if human testimony were more reliable. Patients and their friends so often deliberately suppressed

the truth or for some reason made false statements, that this class of evidence always had to be received with great caution. When reliable, such evidence was of great value in diagnosis, and formed a diagnostic barrier between this degraded type of small-pox and the other disease which it most resembles—chicken-pox.

It is usually rather gradual in its onset, and is marked by frontal headache (which, according to the statements of patients, is always the most prominent symptom), general febrile symptoms, backache (in about 20 per cent. of the cases), vague general pains, vertigo, rarely distinct rigors, and sometimes vomiting. Patients almost always liken the attack to one of influenza. The invasion period varies from one to ten days, and averages about five days, and during this time the toxæmic fever may run rather high. One had naturally very few opportunities of observing the disease at this stage, but in instances of the more severe cases which one has personally seen, and in others upon which the testimony of other medical practitioners has been forthcoming, the temperature in the invasion stage has mounted well above 103° F., and sometimes reached 105° F.

All the symptoms of the period of invasion usually remit for a variable time before the rash appears, and the patient considers himself quite well and generally returns to work, if the illness has been sufficiently severe to compel him to cease work at all; for in some of the milder cases the severity of the symptoms is not sufficient to cause cessation of regular occupation. In some cases the symptoms of the invasion period do not abate until the rash appears, but this seems to be unusual. Speaking generally, the period of invasion is longer than in the classical type of the disease.

Judging from the cases in which young children have been affected in this epidemic, the invasion period is much less marked in them than in adults in all its manifestations.

Machinery for Controlling the Epidemic.

The machinery for controlling the Sydney epidemic was twofold in character. The Commonwealth Quarantine Service established a sufficient medical and nursing staff at the Quarantine Station at the North Head of Port Jackson under the control of Dr. C. W. Reid, Chief Quarantine Officer for New South Wales, to which all patients known to be suffering from small-pox were conveyed by steam launch

from the city. Here they were treated in the Quarantine Hospital and detained until they were judged to be no longer infectious.

A special visiting medical staff was formed at the offices of the New South Wales Department of Public Health. Every case of suspected small-pox reported by a medical practitioner or by any member of the public was visited by a member of this staff, and if the case was diagnosed to be one of small-pox the patient was conveyed by motor ambulance to the depot at Woolloomooloo Bay, where he was embarked on the launch. All medical practitioners were notified by circular of the obligation on them to report every case of illness which might reasonably be suspected of being small-pox, and the citizens generally were warned to the same effect through the public press.

A very large number of persons were seen and examined by the visiting medical staff who proved not to have small-pox. More than 3,000 cases of chicken-pox alone (which has been unusually prevalent in Sydney during the year 1913) were thus seen. As soon as the staff had obtained some experience with the particular type of small-pox of this epidemic the diagnosis from chicken-pox presented, as a rule, very few difficulties, and subsequent results showed that the number of mistakes in diagnosis was small.

During the epidemic the method of treating contacts was twice modified. All through the epidemic, the visiting medical staff on diagnosing a case of small-pox offered vaccination to all contacts. This practice was never changed. But at the beginning of the epidemic all house contacts were also conveyed to the Quarantine Station and detained until vaccination was proved successful. Later on this policy was temporarily modified owing to the impossibility of housing all the contacts at the Quarantine Station during the height of the epidemic. During this period only those contacts who refused vaccination were deported to North Head. At a still later period, when the daily number of cases had fallen to two or three, the practice of removing all contacts to quarantine was again put in force.

The Influence of Vaccination.

The evidence gained from the epidemic of small-pox in New South Wales is a striking vindication of the value of vaccination. Our experience has been that whether one insertion or more were made, whether the area of the vaccination cicatrix were as large as a florin or as small as a pin's head, so long as a typical vaccine vesicle, followed by a

foveated pigmented cicatrix, was produced, the protection afforded against small-pox was absolute for the time being. The whole epidemic lasted for a period of time too short to afford any evidence as to the duration of the protection.

In reviewing the available evidence as to the efficiency of vaccination in protecting against attack, it will be apprehended that that class of evidence has the greatest value which deals with the staff which had the control of the epidemic, because not only were the members of that staff more exposed to the risks of infection than the public generally, but they were constantly so exposed. Moreover, their vaccinal state could be easily and certainly ascertained.

The staff employed by the Commonwealth of Australia at the Quarantine Hospital, where nearly all the metropolitan patients were treated, numbered fifty-two, of which four were medical officers and forty-eight were nurses or hospital attendants. All the above were successfully vaccinated or re-vaccinated at the outbreak of the epidemic and none of them contracted small-pox. The staff employed by the New South Wales Health Department and coming into dangerous relationship with infectious cases numbered fifty. Seventeen of these were medical officers employed in visiting and diagnosing suspected cases; five were sanitary inspectors; four were members of the disinfecting staff employed in disinfecting houses from which small-pox patients had been removed; twenty-four were nurses who were employed in nursing country cases and a few metropolitan patients who for various reasons could not be removed to the Quarantine Hospital. The whole of this staff were successfully vaccinated or re-vaccinated at the beginning of the epidemic, excepting one member of the disinfecting staff. This man managed to evade the general instruction which had been issued to the staff to undergo vaccination. He had never been vaccinated in his life, and *he alone of the whole two staffs contracted small-pox.*

Among those persons who suffered from small-pox the vaccination statistics were as follows: Sixty-seven persons stated that they had been vaccinated in infancy, but in only forty-six of these were any vaccination scars visible. All of these sixty-seven persons were adults, and the majority were over 40. Among them were two persons who claimed to have been re-vaccinated—one of them thirteen years and one twenty years before being attacked by small-pox. In addition to those persons said to have been vaccinated in infancy, two of those attacked in the Sydney epidemic claimed to have been vaccinated

previously—one thirteen years and one fifteen years before they were attacked. One of the two in this last group showed a vaccination cicatrix, but in the other case there was no such mark to be found.

Passing to the subject of recent vaccination, it was found that with two doubtful exceptions, no person who had been successfully vaccinated during the course of the epidemic afterwards contracted small-pox. By this one does not mean to say that no person who had undergone the operation of vaccination afterwards contracted small-pox. On the contrary, this happened in sixty-one instances. But with the two doubtful exceptions above referred to, in all the sixty-one instances the vaccinations had either been followed by no reaction whatever (thirty-nine cases), or had resulted in the aberrant reaction which Seheult has christened a "mulberry" (twenty cases), a reaction which, when it follows vaccination in a person who has not already had small-pox, appears to be due to defective lymph. A vaccination followed by this reaction does not afford protection either against subsequent vaccination or against small-pox. The two doubtful cases were those in which vaccination had been performed thirty-eight days and two months respectively before attack by medical practitioners who afterwards gave certificates of successful vaccination, but when seen by us the vaccination scars were not foveated or depressed, and would not have been accepted by a member of the staff of the Department as indicating successful vaccination. The other 938 persons who were attacked during the epidemic had never been vaccinated.

These statements may be condensed as follows: Of the 1,070 persons attacked to December 31, 1913, in the New South Wales epidemic of small-pox, 69, or 6·4 per cent., had probably been vaccinated in infancy or over thirteen years before being attacked; 999, or 93·3 per cent., had never been successfully vaccinated before contracting the infection of small-pox.

The failures in vaccination indicated were due to the inferiority of the lymph used by the Department during one period of the epidemic.

Even these figures, powerful argument as they are in favour of vaccination, do not by any means disclose the whole of the case in its favour. Many instances occurred in the earlier part of the epidemic in which infantile vaccination was quite effective in protecting adults from infection. Cases, for instance, were not infrequent in which all the children of a family (born in New South Wales and therefore unvaccinated) were attacked by small-pox, while the parents who had been born in Europe and vaccinated in infancy escaped infection.

Sydney was probably prior to this epidemic, as regards the younger portion of its inhabitants, the worst vaccinated of any civilised community in the world. At the present time over 50 per cent. of its population is successfully vaccinated and therefore effectively protected against small-pox for some years to come.

Another kind of evidence is that afforded by the results of vaccination of contacts. Over 6,000 persons who were discovered to have come into close contact with persons diagnosed to have small-pox were vaccinated during the epidemic by the medical staff of the Department (quite apart from vaccinations performed in public vaccination depots or by private practitioners), and in all cases in which vaccination was performed within two days of exposure to infection and was successful in producing a normal vaccinal reaction, the vaccinated person was protected against small-pox. The only failures were the instances in which the exposure had been three or more days before vaccination took place, and the latter had therefore been unable to "catch up" with the infection. Our experience in this direction was in accord with the observation of Marson (Reynolds's "System of Medicine,"[1]) that vaccination within two days after exposure to the infection of small-pox will usually avert the attack of the latter disease.

In quite a number of instances during the epidemic the person attacked by small-pox was the only unvaccinated member of his household. Such an event generally occurred in households residing near a dwelling in which a case or cases of small-pox had occurred between two and three weeks previously, and the other members of the household had been vaccinated in consequence as a precautionary measure.

To quote Wanklyn, our experience of the efficacy of vaccination during the Sydney epidemic was that "anyone who is recently and successfully vaccinated cannot by any loss of health, by any degree of exposure, or by any possibility of any kind at all, contract small-pox."

The "mulberry" reaction which has been referred to in this paper as occasionally following vaccination has been already described by Seheult in his account of the Trinidad epidemic of mild small-pox.[2] This reaction may be produced under various circumstances. It was commonly and most frequently seen in Sydney after the vaccination of persons who had already suffered from the Sydney epidemic of small-pox. Vaccination of

[1] Reynolds, "System of Medicine," 2nd ed., 1870, i, p. 268.

[2] *Proceedings*, 1908, i, p. 290.

persons who were suffering, or who had suffered, from small-pox in the vast majority of instances was followed by no reaction; but in a minority of cases a slight and very much modified reaction resulted, which consisted in the delayed appearance at the site of inoculation of a red mulberry excrescence which either dried up without any further development, or which eventually resulted in ill-developed vesicles often containing some blood. There was an absence in these cases of the inflammatory zone round the vesicles and usually a lack of constitutional symptoms. When the vesicle dried up it formed a thin scale, which on falling off left a small red excrescence which gradually became absorbed. No cicatrix finally resulted, but the skin remained stained for some weeks.

A similar reaction to the above was often observed to follow the re-vaccination of persons who had previously been vaccinated (in infancy) but had not suffered from small-pox. It was also occasionally observed in persons who had never either been vaccinated or suffered from small-pox. In these cases it appeared to be due to some defective quality in the lymph, and, as has already been stated, a primary vaccination followed by such a reaction gave no protection against subsequent attack by small-pox. In Sydney most of the instances in which the "mulberry reaction" followed the vaccination of persons who had not had small-pox were consequent upon vaccination done with a particular strain of lymph which was soon discarded on account of its proving inefficient.

It is to be noted here that the vaccination of persons actually suffering from the eruption of small-pox in the Sydney epidemic was occasionally, though very rarely, followed by a good reaction. Such successful vaccinations were done within one or two days after the small-pox rash appeared, and the latest case noted was one in which successful vaccination was performed on the third day after the appearance of the small-pox rash. This observation is in accord with the observations of Ricketts and of Hibbert and Robinson, all of which relate to the classical and more severe type of small-pox.

Infectivity of the Sydney Epidemic of Small-pox.

Infectivity in this epidemic appears frequently to have had a low intensity. For instance, in two cases early in the epidemic before the diagnosis of small-pox was made, persons who suffered from very mild attacks (in one case there were only three or four lesions on the whole

surface of the body) failed to carry infection into their families though they occupied the same bedrooms as other unvaccinated members of the family. In cases accompanied by a copious rash the infectivity of the disease appears to have been fairly intense, and in households unprotected by vaccination, as soon as a diagnosis of small-pox in one member of the family was made the disease has generally attacked all the members of the household. This state of affairs was chiefly observed at the beginning of the epidemic or in families where the earliest case was concealed or overlooked, as later on, when one member of a household was found to be attacked, all the other inmates were protected by vaccination. The slow spread of the epidemic in an almost entirely unvaccinated population was no doubt due to the degraded infectivity. In most cases infection appears to have been conveyed by direct personal contact, and in quite a number of instances young men and women were found to have infected members of the opposite sex with whom they had been "keeping company"; on the other hand, the convection of infection by fomites was not much in evidence. Two striking examples to the contrary were that of a young woman who borrowed a handkerchief from an acquaintance suffering from a mild attack of small-pox, and was herself attacked twelve or thirteen days later; and the case of an old man who slept one night in a bed which had the previous night been occupied by a small-pox patient with whom he had not been otherwise in contact. He also sickened with small-pox about a fortnight later. But in both these cases there were special opportunities for infection. As a rule, the progress of the epidemic could be traced by direct and close personal infection from person to person.

The evidence from the behaviour of the epidemic outside Sydney also points to low infectivity. In the course of the epidemic twenty-seven country towns or districts of New South Wales were invaded by small-pox, and the total number of cases diagnosed in these localities only mounted to fifty-two. The greatest number of persons attacked in any one locality was six, and in sixteen localities only one person was attacked. In all but six of the localities invaded the first person, or the only person attacked, had come from Sydney, where he had been infected. In five instances the evidence of the source of infection was doubtful, and in one instance the person attacked had been infected in New Zealand. In country places, therefore, when the authorities were on their guard and ordinary measures of segregation and vaccination were enforced from the earliest diagnosis of an imported case of small-pox, the disease did not spread.

Conclusions.

(1) The epidemic prevalent in Sydney during the latter half of the year 1913 was a mild type of small-pox, and was identical with the epidemic infectious disease variously known as "Cuban itch," "alastrim," "Spanish measles," "milk-pox," "lumbermen's itch," &c., which has prevailed during recent years in various parts of the Americas. The disease was probably introduced into Sydney from Canada.

(2) Recent successful vaccination conferred absolute protection against infection during the Sydney epidemic.

(3) Besides being mild in its manifestations, the disease had a relatively low infective power, and was as a rule transmitted only by personal contact. Transmission by fomites or by aerial convection rarely occurred.

(4) As regards diagnosis, the only confusion liable to arise is between this disease and chicken-pox. Such uncertainty is only likely to occur in respect of a few of the milder cases of this mild type of small-pox, and it may be overcome by paying attention to the distribution of the focal rash and to the manner of onset, both of which are distinctive from that of chicken-pox. In a minor degree the characteristics of the individual lesion are also of value in diagnosis. For instance, the clear, watery pock which collapses easily on pressure with the finger-nail, with escape of its watery contents, is very frequently present in some stage of an attack of chicken-pox, and is never seen in mild small-pox. Some other characteristics of the focal lesion, such as shape, irregularity of outline, umbilication, and the occurrence of the rash in successive crops, have no great value in the diagnosis of chicken-pox from mild small-pox. At the beginning of an epidemic of mild small-pox diagnosis should be controlled by vaccination results.

(5) The question of prevention centres round that of vaccination. In a well-vaccinated and re-vaccinated community the disease cannot spread. In an unvaccinated community it will certainly spread unless the earliest cases are sufficiently severe to attract attention, because many of the cases are so mild that patients will not regard their attacks seriously, and refuse to seek medical assistance. Small-pox in Sydney was almost entirely confined to the poorer neighbourhoods, because the better educated classes nearly invariably sought vaccination or re-vaccination as soon as the existence of small-pox in an epidemic form was publicly notified.

The Nature of the Recent Small-pox Epidemic in Australia: Microbiological Findings and Animal Inoculations.

By J. Burton Cleland, M.D., Ch.M.Syd.[1]

(Principal Microbiologist),

AND

E. W. Ferguson, M.B., Ch.M.Syd.[1]

(Assistant Microbiologist).

There has been some discussion and doubt in certain quarters as to the exact nature of the recent variola-like epidemic which has occurred in New Zealand and New South Wales. Whilst the various health authorities in Australia and New Zealand are all agreed that the disease was small-pox or variola, some medical men have considered that the complaint was a distinct entity separable from small-pox and chicken-pox. They, and probably quite rightly, considered it to be "alastrim," but the evidence available in connexion with this disease, alastrim, in America seems to indicate that it is unquestionably a form of small-pox. That our disease is also a form of true small-pox we have the following evidence to submit in proof.

General Considerations.

The epidemiological identity of different manifestations of any particular disease is acknowledged when the same specific micro-organism is found to be responsible for the differing effects. Thus in different epidemics of any infectious disease there may be increased or diminished virulence and special manifestations particular to one or the other. An epidemic of plague may be entirely pneumonic or entirely bubonic, yet no one considers that in consequence we are dealing with two separate diseases. Scarlet fever as at present known in Australia is a comparatively mild complaint, thus differing from the severe epidemics known years ago elsewhere. Whilst in individual cases of disease in the epidemic the effects on the patient will vary according to the soil in which the organisms are planted, the seed being of the same

[1] From the State Microbiological Laboratory, Department of Public Health, New South Wales.

strain in each case, in the various epidemics we have a superadded feature, and that is variation, little or great, in the nature of the organism. All living creatures vary more or less in each new generation. Where countless generations occur in the course of a few days the variations might be expected to be great. Strange to say, however, as regards the organisms of infectious diseases in man, the various generations seem to breed true to a remarkable extent in spite of such ample opportunities for differing. Naturally in many cases the only indications we have that they do breed comparatively true is in the organisms producing a very similar condition of disease in their hosts in different epidemics. It is a matter well known, however, that whilst some epidemics of infectious diseases are inordinately severe, others are remarkably mild in spite of the soil being presumably almost identical in each case. If, therefore, one epidemic is decidedly milder, with a negligible mortality, than another, we have no grounds for supposing that merely because it shows these features it is not due to the same disease and the same specific organism as are responsible for the other epidemic with very severe symptoms and a heavy mortality. The mere mildness of the recent epidemic in New Zealand and New South Wales cannot, therefore, alone be considered as indicating that it was not due to the same specific organism as that producing the more fatal small-pox of the Far East. It may be well here to point out that in a general way it is of no advantage to a disease germ to kill or incapacitate its host, as thereby its survival in nature may be rendered the more precarious. It is to the interests of the invading organisms and of the host that as far as possible they should all live together in peace and concord, and that the invading organism should harm its host as little as possible, and the host injure the invaders not at all.

There are now recognised a few diseases which are due to ultra-visible viruses of unknown nature. We would be perfectly unaware of the existence of such viruses were it not that during their life-histories in the different hosts they injuriously affect these to a greater or less extent. It is possible that a number of slight complaints, as for instance many highly infectious febrile complaints, which merely incommode their respective hosts for a few hours or days, may be due also to ultra-visible viruses, and that further we may be surrounded, and in fact living, in symbiosis with vast armies of such organisms, which in the course of ages have so accommodated themselves to their hosts that they produce now no recognisable ill-effects. The organism responsible for variola is probably closely allied to this group.

From these observations it will be gathered that there is probably a tendency for all virulent diseases in the course of ages to become less virulent, mutation in this direction being of advantage to the organism by enabling it to multiply more freely. Therefore from time to time in such diseases as small-pox, measles, and scarlet fever, we should expect to find milder forms arising by such mutations and maintaining themselves in the community more easily than the virulent forms. The present epidemic of small-pox we take to be an instance in point.

Vaccinia, the Present Epidemic, and Small-pox.

Consideration has at times been given to the question as to whether cow-pox is due to the same specific organism as that producing small-pox in man, whether it is a variant of this, or whether it is sufficiently distinct to be ranked as a different species, and yet sufficiently close to be able to produce in its host antibodies capable of protecting against small-pox, this being due to the two organisms having a common ancestry. That cow-pox is actually due to the same specific organism as small-pox seems to be conclusively proved, however, by the fact that material from small-pox patients can be conveyed to calves, and that after a few generations in these animals it produces a disease in them transferable back to man whose course is now identical with that of the natural disease cow-pox. In other words, at will the disease small-pox can be converted into cow-pox, though the converse cannot be brought about. We are in the dark at present as to how the high contagiousness of small-pox is thus lost on transmission to the cow. It has been suggested that in the complicated life-history of the organism only a certain phase is capable of taking place in the cow, and that once this loss has been achieved for a few generations the hereditary tendency for it to reappear on conveyance back again to the human host is lost. This, however, is beside the question before us.

The cases seen in the present epidemic seem to us to exhibit a "halfway house" stage between ordinary severe small-pox and vaccinia. Though the disease is contagious, it is not as markedly so as ordinary small-pox. Though the patient may be covered from head to foot with pustules, the secondary fever is remarkably mild when compared with semi-confluent or ordinary small-pox. The large number of cases in which only a few pocks developed indicate that the white race has a comparative immunity to this disease as compared with ordinary small-pox, but that this immunity is less than that

to generalised vaccinia. The impression has also been left upon us that the skin lesions are less deep than in ordinary small-pox. By analogies, therefore, with vaccinia, the conclusions we seem justified in coming to are these:—

That the present epidemic is due to the same specific organism as small-pox; that it is probably due to a mutational change in this organism by which its virulence is diminished, and possibly some phase of its life-history eliminated; and that it is probable that it is unlikely to recover the general intense virulence of ordinary small-pox.

Cytorhyctes variolæ.

During the course of the epidemic opportunity has been taken to search for the supposed parasite of small-pox, *Cytorhyctes variolæ*. Various investigators have detected in the epithelial cells and also in the nuclei of these cells various bodies which they have considered as foreign to the host cells, and as representing either actual parasites or a reaction in the cells to the presence of such bodies, the parasite itself being hidden as under a cloak in the centre of the reactionary area (Chlamydozoa). Unfortunately, all the cases in which it was possible to obtain material showed already considerable leucocytic infiltration of the pocks. As the supposed parasites are most easily recognised in the stage preceding this, investigations in this line were naturally hampered. Nevertheless, in one case, by Ross's new jelly method of investigation of living cells, bodies were seen in the protoplasm of what appeared to be epithelial cells identical with the descriptions of *Cytorhyctes variolæ*. Unfortunately, there is no distinctive reaction of these bodies, as for instance in the shape of chromatin, specifically to identify them as organisms and not cell products. One cannot, therefore, be certain that the bodies seen were not degeneration products of the cells. In preparations made by the wet method of fixation, either by osmic acid vapour or by corrosive sublimate — acetic acid, and stained with iron hæmatoxylin or Giemsa's stain, nearly all the cells met with consisted of leucocytes. In these cells foreign bodies were also seen from time to time which may have been engulfed Cytorhyctes, or may have merely been further degeneration products.

Tissues from Small-pox Patients.

The tissues that we have had an opportunity of examining have consisted of pocks from the skin, and the placenta and some of the organs of a patient who died suddenly during the course of delivery on the fourth day after the eruption had first appeared. The following is a description of the appearances met with:—

Pocks in the Skin. — Sections through a pustule showed areas of various sizes in the skin, as a rule larger near the surface and extending to the rete Malpighii, due to the separation of the epithelial cells by a transudation of lymph accompanied with an exudation of leucocytes. In some instances the pustule was broad, and almost as wide near the rete Malpighii as at the surface, whilst in other cases it showed a more wedge-shaped arrangement with a wide base narrowing towards the apex, exhibiting a kind of radiating appearance. In the deeper pustules vesiculation had separated the Malpighian cells from the cutis vera, but did not seem to have extended deeper; although there was some polymorphonuclear infiltration of the cutis vera immediately below the vesicle. The polymorphonuclear leucocytes showed in great part nuclear degeneration and fragmentation. In many the nuclear fragments assumed the appearance of rounded, scattered bodies, dark on one side with a lighter ring on the other, and of varying size. Further sections were stained by Giemsa's method, and were also put through Levaditi's process, but no definite parasitic bodies were detected. A special search was made for spirochætes.

The kidney, *liver*, *spleen*, and *placenta* were also examined by these latter methods, but with negative results. In the kidney the cells of some of the convoluted tubules were granular with indistinct outlines and somewhat swollen. Apart from this nothing abnormal was detected, as was the case also in the liver and spleen.

The placenta showed some interesting changes. There was nothing very remarkable to the naked eye about it in its preserved state as received by us. Some dark patches under the surface appeared to be dilated veins or blood spaces. Sections showed a polymorphonuclear infiltration in parts, and also areas in which the chorionic villi had definitely undergone cell necrosis. There were also areas of blood extravasation, and also numerous places in and around the villi, which showed nuclear fragments without definite cell outlines. The appearances presented suggested that, through the cutting off of nutrition to

the villi some of these had undergone degeneration and consequent invasion by polymorphonuclear leucocytes. This diseased condition of the placenta seems to afford a reasonable explanation for the frequency of abortions in the present epidemic. As the epithelium of the chorionic villi has the same epiblastic origin as the surface epithelium of the body, special search was made to see whether any changes could be detected suggestive of a condition equivalent to the pustules found in the skin. Perhaps the polymorphonuclear invasion may be associated with such a change.

The histological description of the skin lesions found in this case are strictly comparable with those described for true small-pox.

Animal Experiments.

With some doubtful exceptions, attempts to convey chicken-pox to animals have signally failed. On the other hand, the inoculation of material from small-pox patients has proved successful and strains of vaccinia have been derived from such a source. It was therefore obvious that light might be thrown upon the nature of the present epidemic by an attempt to convey material from the pustules to calves. Such conveyance, if successful, would undoubtedly exclude the possibility of the disease being a form of virulent chicken-pox.

During the course of the epidemic material was obtained from four patients about the fourth or fifth day after the first appearance of the rash—the material from each patient being kept separate. The lesions were in the condition of pustulation. The material thus obtained was conveyed to an isolated shed far removed from human habitations or the vaccine establishment. The first procedure was the inoculation of a bull calf on October 10. The material obtained, after dilution with approximately an equal amount of pure 50 per cent. glycerine and water, was applied to small scarified areas on the abdomen of the calf. At the end of four days the areas were covered by fine yellow flaky crusts. There was no definite œdema or swelling, but some slight redness was present round one area. As a control to these four areas a further area had been scarified and glycerine and water alone rubbed in. All four inoculated areas did not show the same degree of reaction, one being decidedly less. It was doubtful from this experiment whether any definite "take" had been obtained, though this seemed probable. Two days later a second calf, a heifer, was similarly inoculated with material scraped from the previous case and emulsified in glycerine and

water. Four days later the inoculated areas showed dried, scaly crusts more decided than in the first calf. These were slightly adherent, but there was no redness or œdema about them and no definite swelling. They were scraped and the material preserved for further inoculations. Five days after scraping a re-examination of the areas showed in their neighbourhood several small papules, one of which had become a pustule. It seemed from this experiment that a "take" had been secured, though the result was still slightly indefinite.

On November 7 a further calf was inoculated on four areas with the four different strains. At the end of four days there was a very definite reaction. The lines of scarification were showing up well. There was a moderate amount of scabbing with, in some of the cases, decided redness and swelling and the presence of vesico-pustules. There was now no doubt at all that a successful "take" had been established.

A fourth calf was variolated with material of the third remove on December 11, and took as in typical vaccinia, and on January 15 the fifth remove was obtained from calf 6, when a large area was inoculated for the purpose of obtaining seed. The "take" in this instance was indistinguishable from that of typical vaccinia. It would therefore seem that in the fourth remove and onwards the reactions obtained were indistinguishable from those of vaccinia.

In addition to this attempt to produce typical vaccinia in calves from material obtained from the small-pox patients, these same animals were made use of to test further the relative protection obtained from the varioloid vaccine against vaccinia and vice versa. Much interesting information has been obtained in this way, and it may be well first of all to summarise the results obtained from the various calves experimented with and then to discuss the bearing of these results.

SUMMARY OF INOCULATIONS MADE ON CALVES.

Calf 1.

October 10.—Variolated directly from four human cases on four small scattered white areas, each about ¼ sq. in. in superficies.

Result.—A slight, somewhat doubtful "take."

October 21.—Vaccinated over an area about 3 in. by 1 in.

Result.—Good typical take.

November 7.—Re-variolated with material from first and second removes over two small areas, each about 1 sq. in. in area.

Result.—Definite slight take.

November 24.—Re-variolated with the second and third mixed removes over two small areas each about 1 sq. in. in surface.

Result.—No definite take.

December 11.—Re-variolated with the third remove, over two similar small areas.

Result.—Definite slight take.

December 30.—Re-vaccinated over an area about 3 in. by 1½ in.

Result.—Definite slight take.

Calf 2.

October 16.—Variolated with the first remove over four small areas, each about 1 sq. in. in extent.

Result.—A definite slight take, more decided than in the case of calf 1 on October 10.

November 7.—Re-variolated with the first and second removes over two small areas of about 1 sq. in. in extent.

Result.—Moderately good take.

November 24.—Re-variolated with the second and third mixed removes over two similar small areas.

Result.—No definite take.

December 11.—Re-variolated with the third remove over one area of about 1 sq. in. in size.

Result.—Definite moderate take.

December 30.—Vaccinated over an area of about 3 in. by 1½ in.

Result.—Definite moderate take.

Calf 3.

November 7.—Variolated with the second remove over four small areas each about the size of 2 sq. in.

Result.—A take almost as good as in typical vaccinia.

November 24.—Re-variolated with the second and third mixed removes over two small areas as in calves 1 and 2.

Result.—A slight but definite take.

December 11.—Re-variolated with the third remove as in calves 1 and 2.

Result.—Definite moderate take.

December 30.—Vaccinated as in calves 1 and 2.

Result.—A slight take.

Calf 4.

December 11.—Variolated with the second remove mixed lymph, the third remove mixed lymph, and the four separate lymphs of the third remove, over six areas, each about 2 sq. in. in extent. The total area variolated was about 12 sq. in.

Result.—A take as in typical vaccinia.

December 30.—Vaccinated.

Result.—No definite take, crusts of the previous variolations still present.

Calf 5.

November 24.—Vaccinated for the purpose of preparing vaccine lymph over an extensive area, probably amounting to 60 sq. in.

Result.—A typical result.

December 17.—Re-vaccinated on the side.

Result.—No take, but it should be noted that this was an unfavourable site for re-vaccination.

January 9.—Variolated over an area of about 3 sq. in. with the fourth remove.

Result.—Fairly definite slight take.

Calf 6.

January 15.—Variolated with the fourth remove, mixed, over a large area of the abdomen amounting to about 50 sq. in.

Result.—A good take as in typical vaccinia.

Monkey.

November 15.—Variolated on the arm, as is done in vaccinating children, with lymph of the third remove consisting of strains from two patients and the mixed strain from the four. This monkey took as in an ordinary vaccinia. There were some secondary papules about the primary lesions; no generalisation occurred.

January 22.—This monkey and another fresh monkey were each vaccinated on the arm with ordinary vaccine lymph in three places. On the ninth day the new monkey showed on each of the areas adherent, yellowish-brown, heaped-up scabs on a slightly indurated base with a slight surrounding red flush. *Two of the areas on the original monkey's arm showed normal skin* with the remains of the scratches, *but the third spot showed a heaped-up, yellowish-brown, mulberry-looking scab* the size of a three-penny piece. This occurrence after variolation of a mulberry reaction in one area and of no reaction in the other two, is of very great interest.

From a study of these animal experiments the following points may perhaps be deduced:—

(1) On direct conveyance of a small amount of material from a recent human pustule to a calf the "take" is at first very slight; is more definite but still slight in the second remove; is marked in the third remove, and assumes the appearance of typical vaccinia in the fourth remove. How far these progressive changes are due to the small amount of material available for the first and second inoculations, and how far to an increased accommodation of the organism to the new host is impossible to say. Calves 3 and 4 seem to indicate that there was such a progressive accommodation.

(2) A very slight, imperfectly variolated area will not apparently modify a subsequent vaccination.

(3) Such a subsequent vaccination, if only over a small area, will modify but will not entirely prevent successful results on re-variolation and re-vaccination (calf 1).

(4) An extensive area of recent variolation may completely protect against subsequent vaccination (calf 4).

(5) An extensive area of vaccination will profoundly modify attempts at variolation six weeks later, but may not absolutely prevent all reaction.

(6) Variolated lymph of the third remove can produce an appearance in monkeys resembling typical vaccinia unaccompanied by any generalised eruption.

(7) It is very obvious that the size of the area variolated or vaccinated

is a very important factor as regards complete protection against subsequent variolation or vaccination or vice versa. The time factor and the degree of reaction also probably play a part.

(8) The results obtained from calves whether variolated or vaccinated seem to be identical.

Conclusions.

From the above given microbiological data we have attempted to show that the present disease is a form of true variola. We have come to this opinion on the following grounds :—

(1) Such a modified variola was what on microbiological grounds might have been expected to arise.

(2) The disease in some of its features seems to be intermediate between ordinary variola and vaccinia, though so close to the former as to be in most respects indistinguishable.

(3) Bodies resembling the *Cytorhyctes variolæ* have been seen in material from cases.

(4) The appearance of sections of tissues from cases accord with those described from small-pox patients.

(5) The disease is conveyable to calves by inoculation.

(6) The condition thus produced behaves as does inoculated vaccinia.

(7) This inoculated disease and vaccinia are mutually more or less completely protective against each other provided that sufficient area be inoculated.

FIG. 1.

Sydney outbreak. H. M., female, aged 14, unvaccinated. Photographed on fifth day of rash. This case represents the prevalent mild type of the epidemic.

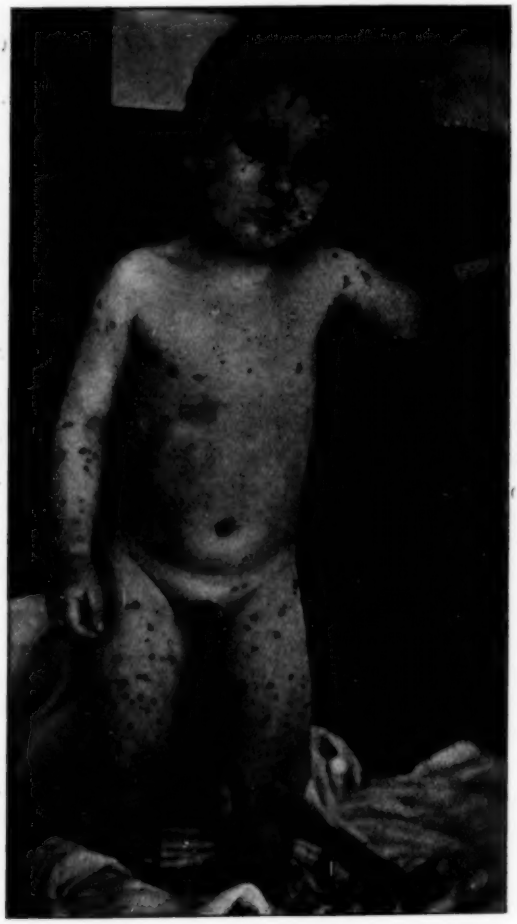


FIG. 2.

Sydney outbreak. R. W., male, aged 2¼, unvaccinated. Photographed on fifth day of rash. Mild case.

FIG. 3.

Sydney outbreak. H. W., male, aged 30, unvaccinated. Photographed on sixth day of rash. Severe case.

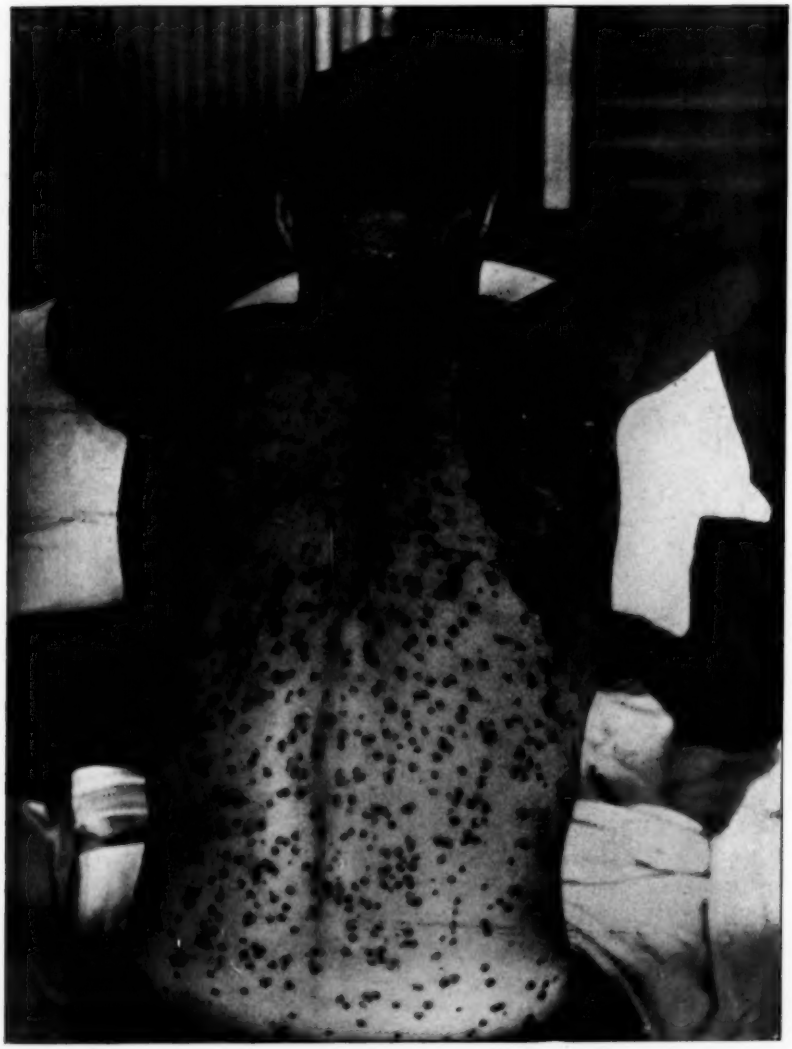


FIG. 4.

Sydney outbreak. H. W., male, aged 30, unvaccinated. Photographed on sixth day of rash. Severe case.

DISCUSSION.

The PRESIDENT (Dr. C. J. Martin): The papers communicated by Dr. Armstrong and Drs. Cleland and Ferguson are very welcome. Most of us have watched the progress of this epidemic in Sydney with interest, firstly as an extensive outbreak of small-pox occurring amongst a practically unvaccinated population, and, later, on account of its unusual benignity. I do not propose to take up any length of time with my remarks, as I see there are a number of experts on small-pox present. We also have the advantage of the presence as guests of Dr. Culpin, who was actually concerned in the smaller outbreak at Brisbane, and of Dr. Norris, formerly Director of Public Health for the Commonwealth of Australia, who will be able to supply any information as to the local conditions obtaining in Sydney. Dr. Armstrong's clear account of the epidemic, accompanied by his very excellent photographs, leaves little doubt in my mind that the Australian epidemic is one of the same disease as has been so prevalent in Brazil during recent years, and called locally alastrīm. A good account of this disease appeared a few years ago in a report by Dr. Ribas, Chief Sanitary Officer of the State of São Paulo. The subject has also been recently dealt with by Dr. Aragão in the "Memorias do Instituto Oswaldo Cruz," 1911. According to the latter 250,000 cases have occurred in the last few years in five or six of the States of Brazil. The epidemic is still prevalent amongst the population of unvaccinated natives in the hinterland, who continually infect the more civilised portions and towns. Dr. Ribas came to the conclusion that the disease was not small-pox, but identical with the South African milk-pox, or amaas, for the following reasons: (1) Its low mortality (0·5 per cent.), whereas the usual mortality from small-pox under similar conditions is about 60 per cent.; (2) absence of secondary fever; (3) absence of a characteristic smell of small-pox; (4) non-occurrence of deep scars; and (5) the short duration of the immunity against vaccinia, it being possible to vaccinate the larger proportion of persons who have suffered from the disease within six months of their recovery. Drs. Cleland and Ferguson's experimental investigation into the cross-immunity between vaccinia and the virus of the Sydney outbreak are interesting. They appear to have been successful in inoculating calves directly with the material. This, if my memory serves me, Aragão was unable to do in Brazil.[1] The diseases variola, alastrīm and varicella may be presumed to be of common parentage, and to have become differentiated and at present breed true. It seems to me very desirable that the cross-immunity relations between the Sydney virus and variola should also be investigated, but this could obviously not be done by the authors in Sydney.

Dr. J. C. MCVAIL: The papers to which we have listened to-night form a valuable contribution to the epidemiology of small-pox, and we are much indebted to those who have sent them from the confines of the Empire for

[1] It is difficult to get small-pox to take directly on the cow.

consideration at the centre. That they are submitted here illustrates incidentally the nature of the relations which exist between Britain and her Colonies. In the history of small-pox mild types or "sports," as they might be called, have been recorded from time to time. One old writer declared that there was a kind of small-pox which a doctor could not cure and another kind which a nurse could not kill. In 1789, at a medical society of which he was a member, Jenner discussed a remarkably mild epidemic of small-pox which was prevalent in his part of the country. Vaccination had not then been introduced, and the question which particularly interested him was whether this much modified small-pox could yield for purposes of inoculation a material which would produce even a slighter disease than that resulting from the small-pox inoculation practised at the time—a practice which had been greatly modified and improved as compared with methods which, half a century earlier, had brought small-pox inoculation into temporary disrepute. Again in 1806, Dr. Adams, of the small-pox hospital in London, reported on an abnormally mild epidemic of the disease which he designated by the name of pearl-pox. I do not recall any further reference to exceptionally mild small-pox in epidemic form until we come within the past twenty years. The two great epidemics in Jennerian times have been those of 1838-40 and of 1870-73. In both of them, apart from vaccination, the type was virulent with a heavy fatality rate, and it is these epidemics which have been the basis of modern medical opinion as to the normal severity of small-pox. If a trivial disease, such as has occurred in New South Wales, were now to replace throughout the world what has come to be regarded as the standard type of small-pox, the public and professional attitude towards the problem of its prevention would inevitably be altered. A malady as slight as chicken-pox would not have directed against it all the measures appropriate to a dangerous and fatal infection. It seems impossible to say with any approach to accuracy what has been the frequency of mild small-pox epidemics. In early times there would naturally be risk of confusion between small-pox and chicken-pox, but both diseases would be so well known to Jenner and his contemporaries that the variolous character of the epidemics of 1789 and 1806 may safely be assumed. Thomson, of Edinburgh, however, in 1820, maintained the hypothesis that chicken-pox and small-pox were varieties of the same disease. In recent years various epidemics of a mild variety of small-pox have been reported, principally in America. At home, in Leicester and the Midlands of England, there have been visitations of mild small-pox approaching the American variety, though in London the type has been more virulent, as if infection had been derived from the Continent of Europe where, I think, one has not heard much of the modified form of disease. As pointed out by Dr. Armstrong, the New South Wales epidemic has been unusually mild with only a single death in over 1,000 cases, and that hardly attributable to the small-pox attack.

In a very interesting section of his paper Dr. Armstrong deals with the infectivity of the Sydney epidemic, which he points out appeared frequently to have been low, and he attributes to this the slow spread of the epidemic in

an almost entirely unvaccinated population. His view is supported by what he says regarding the experience outside of Sydney, where in twenty-seven invaded towns or districts the total number of cases diagnosed was only fifty-two, the greatest number in one locality being six, whilst in sixteen localities there was only a single case. It seems to me that Dr. Armstrong is sound in the opinion in which he indicates that, other things being equal, the degree of infectivity relates to the amount of the eruption. Unless where there is close personal contact (as between B. E. and E. D., reported by Dr. Armstrong), cases of small-pox presenting a very scanty eruption, say a few pimples only, are likely to exhibit their infectivity almost wholly within the four walls of the dwelling. One instance particularly impressed itself on me. In a small house of two or three apartments two cases of small-pox were reported. They had been infected from a member of the same household, a woman with a very mild attack accompanied by an eruption of only a few vesicles. The nature of her illness had not been suspected; in fact, she had hardly any illness and had gone freely about mixing with the public in a town of 14,000 inhabitants. I awaited the result with some anxiety, but apart from the two members of her own household not a single individual was attacked and no outbreak of the disease occurred. In two cases recorded by Dr. Armstrong there was not even that amount of infection, both failing to convey the disease to unvaccinated persons sharing the same bedrooms. The factory outbreak in which it seems that nearly the whole of 200 operatives were more or less affected must also have been wholly or mainly due to the spread of infection within a building. It is an interesting question whether mild small-pox is more or less to be feared than severe small-pox as a means of conveying the disease. On the one hand, severe small-pox is more likely to be diagnosed and to be subjected to early isolation. On the other hand, mild small-pox, even if undiagnosed, has much less power of infection. My own view is that the balance of advantage lies decidedly with mild small-pox. It is quite true that a medical officer may report that some spread of small-pox infection resulted from an unsuspected mild case of the disease, and that had the case been diagnosed at the beginning there would probably have been no outbreak. That may be freely granted, but it leaves open the question whether if the case had been characterised by an abundant eruption it would not have run greater risk of spreading the disease before removal to hospital, especially if it occurred in a community where vaccination had been neglected or where some proportion of contacts could not be persuaded to submit to the protective operation. Moreover, a plentiful eruption of small-pox may occur without the disease being diagnosed. A tramp, careless of himself and of others, may fight through the initial fever without sending for a doctor, and, experiencing the relief from malaise which usually occurs with the appearance of the eruption, he may travel about the country spreading infection, whilst if the eruption is observed it may be set down to drink or ill-living or the like. In connexion with the low degree of infectivity of mild small-pox no distinction can, I think, be drawn between natural mildness and the mildness artificially resulting from vaccination. The former, however, will breed true, and any resulting cases will commonly be

mild, while small-pox of which the mildness is due to vaccination will quite likely produce a severe attack in an unvaccinated person. Any one who regards severe small-pox as less likely to spread infection than mild small-pox will thus probably have the desired severity merely deferred to the second generation. Concerning the value of success or failure of vaccination as a test of susceptibility or insusceptibility to attack by small-pox, it is necessary to exercise caution. When small-pox inoculation was in vogue, before the days of vaccination, there is evidence that successful inoculation did not necessarily prevent at some later date a more or less complete local success of a second inoculation, without there being any reason to believe that the individual had again become susceptible to attack by small-pox. The virus of small-pox could be cultivated on the skin whilst there still remained immunity of the system against infection. Similarly, there may be susceptibility to vaccination whilst yet the individual is immune to small-pox. At first vaccination, after it has run its course, ordinarily gives absolute protection alike against vaccination and against small-pox attack. Reaction, however, to the insertion of vaccine lymph may return earlier than vulnerability of small-pox infection. When this renewed susceptibility to local vaccinia first begins, the course run by the vaccinal process will be much shorter than normal. Every stage will be hastened. That also applied to a second small-pox inoculation. At a longer interval after a first vaccination, the local reaction would make more approach or fully attain to the normal, but yet, in the absence of this second vaccination, the individual would ordinarily have remained for some time longer safe from liability to small-pox infection. On the other hand, failure of an attempt at vaccination, many years after a primary vaccination, does not necessarily mean insusceptibility to attack by small-pox. It is well known that an attempt made one week may fail, but that a second attempt a week or two later may succeed. When small-pox prevails therefore, the vaccinator should not be satisfied with one effort to provide vaccinal protection. Dr. Armstrong's paper teaches the usual, indeed the invariable, lesson of the very great value of recent vaccination, and the experience of the staff of the Quarantine Hospital affords the usual control experiment. One member of the disinfecting staff evaded the general instruction to undergo the operation. He, and he alone, contracted the disease. Also the outbreak had the usual effect in persuading a large part of the invaded community to accept vaccination, so that at the period dealt with in the paper about 50 per cent. of the population of Sydney had resorted to the Jennerian prophylaxis.

The papers suggest many other interesting points for discussion, but I have already occupied too much of the time of the meeting and will leave others to deal with other features of the epidemic.

Dr. M. CULPIN: I had some personal experience of this epidemic, and to me the question of the identification of the disease is not an easy one to answer. The photograph of a man shown on the screen is a picture of confluent small-pox—such a case as would end fatally. Yet I saw that patient, or one with a similar rash of equal severity, and the man, at the height of the

eruption, was growling at his insufficient diet and complaining that his feet were sore when he stood on them. With the exception of the woman who aborted, there was no fatal case, and I might go further and say that no one was ill after the prodromal symptoms, and this in an epidemic of over a thousand cases. In some cases the distribution of the rash was that of small-pox, but in many this was not so; after examining several score of patients in the Quarantine Station at Sydney, purely with a view of learning how to recognise the disease, and being aided by the very zealous house-man in charge, the only general conclusions I formed as to the distribution of the eruption were that the inside of the upper arm and the part of the chest wall in contact with it were mostly unaffected; that the bare area behind the ear did not often escape and there was a special predilection for the nose; that the abdomen was lightly affected and in a fair number of cases the rash was certainly most marked on the extremities. The individual lesions varied widely: in some cases they were uniform in appearance, rounded and fairly deep-seated, and in these cases the distribution tended to be that of small-pox; in others the lesions were multiform and suggested successive crops; in some, particularly in children, differentiation from varicella seemed very difficult, though I had no doubt as to the correctness of the diagnosis. The proportion of children affected seemed small, young adults making up the greater number of the inmates. We have had epidemics of mild small-pox in this country with few fatalities, but, I ask in all the humility of ignorance, did we see cases of confluent or semi-confluent type without severe constitutional disturbance? We have historical records of numerically severe epidemics of small-pox with a low morbidity and low mortality, but that fact is of no assistance in forming a judgment as to what this disease may be. These records may refer to the disease we are discussing, and what our ancestors called it should not influence us. I have carefully read descriptions of the disease called *alastrīm* in South America and it seems to be identical with the disease we are discussing; whether alastrīm and small-pox are identical is really the question, and if they are, then we may expect sooner or later to see a change of type from what is now a most benign disorder.

Dr. E. W. GOODALL: I am very glad that the Section has had the opportunity of discussing the recent outbreak of small-pox in New South Wales, because the epidemic is one of great interest and importance, and no authoritative account of it has appeared up to the present time, so far as I am aware, in this country. Opinions have been expressed that the disease in question was some disease other than small-pox, and the two principal arguments against small-pox have been the mildness of the attacks and the low infectivity. But no one who has made a study of the history of epidemic disease would attach much importance to these arguments. Curiously enough, the very same reasoning was employed a few years ago by Dr. Brill, of New York, to support the view that a certain disease, of which he had seen a considerable number of cases in the course of some years, was not typhus fever. Clinically, he admitted that the disease could not be distinguished from the

typhus described by Jenner, Murchison, and other writers. But Dr. Brill had never seen a fatal case and he had met with no evidence of its infectivity. Hence there was a new disease, "Brill's disease," added to our nosology. Further clinical and experimental evidence, however, has gone to show that "Brill's disease" is nothing more nor less than typhus.

It is to be hoped that figures giving the exact ages of all the cases will be published. It is of extreme interest to know that the disease had been confined almost entirely to adults, and that it had been so restricted in an unvaccinated community. The evidence afforded by epidemics in the United Kingdom for the last century is to the effect that amongst the unvaccinated the attack-rate and death-rate are high in children. And, referring to pre-vaccination times, such evidence as the Geneva death records and the London Bills of Mortality goes to show that small-pox was a children's disease and was especially fatal to children. But there is evidence that such has not always been the case. Dr. Creighton, in his "History of Epidemics in Britain," quotes writers of the seventeenth century to show that small-pox was at that period not a disease which was dangerous to children. He has also brought forward some evidence to the effect that small-pox was at that time more prevalent amongst adults. The evidence, however, is open to the objection that it was possibly selected, though doubtless unconsciously, and that the statistics are very imperfect, indeed almost wanting altogether. The Sydney outbreak, therefore, though very exceptional in the matter of age-incidence, is probably not unique. Another point of interest is the relationship between malignancy and infectivity. This is an epidemic which might be cited in favour of the view that these two factors vary directly with one another, that the more highly infectious a disease is, the more it is likely to afford a high proportion of severe and fatal cases. But that, again, is a proposition which cannot be sustained in the light of past experience. Anyone who studies the statistics relating to the notifications and recorded deaths which have been published for upwards of twenty years past by the Medical Officer of the London County Council, must come to the conclusion that infectivity and malignancy do not go together. I am tempted to draw from this evidence, indeed, the conclusion that bacterial organisms are furnished with two quite separate powers, that of infecting and that of injuring.

The Sydney epidemic is another proof that there is a very mild type of small-pox abroad in several parts of the world at the present time. Instances of its occurrence in other quarters are given in this paper. To them may be added the disease known as amaas in South Africa, about which there was a controversy a few years ago, but concerning which there is no reason to doubt that it is small-pox of a mild character.

Dr. G. S. BUCHANAN: The Secretaries have received many letters from members expressing their appreciation of the papers and their regrets that the circumstances of the time do not allow them to attend the discussion. Dr. Millard, of Leicester, has sent comments (appended below). Dr. Copeman points out that the leading features of the Sydney outbreak had a close parallel

in an epidemic at Cambridge which he investigated some years ago, where for some time the disease was suspected to be chicken-pox, but proved to be small-pox of low infectivity and mild clinical character. Dr. Niven, in course of a letter, speculates whether it is possible that small-pox is taking refuge from the amount of attention which it is receiving at present, as it seems reasonable to suppose that if the most severe cases of a disease are systematically isolated, the milder type would have a chance of establishing itself. He thinks that this would involve, however, an increased risk of propagation, and once the disease had become prevalent the tendency would be towards increasing the severity of the cases. Dr. Niven adds that the failure of small-pox to propagate itself has often been observed, and suggests that possibly certain cases have special power to disseminate infection, though he has not observed that greater infectivity is more characteristic of severe than of mild cases.

Personally I think that the epidemic described must certainly have been one of small-pox. I am puzzled by the simultaneous excess of chicken-pox and at the ease with which, according to the papers, the two diseases seem to have been distinguished when once the fact had been established that small-pox was prevalent as well as chicken-pox. An analysis of all the cases in age-groups would be particularly valuable, both as to small-pox and as to chicken-pox. It is noteworthy that no cases are mentioned of cross-infection between the two diseases or of small-pox supervening on chicken-pox and *vice versa*. If as a fact a large proportion of the excess of chicken-pox was contributed by adults I should suspect much of the chicken-pox to have been really small-pox.

As to the protection of recent vaccination being the same irrespective of the area of vaccinated surface, it is important to note that Dr. Armstrong is referring only to immediate and temporary protection. It is not to be expected, if home experience is any guide at all, that in primary cases the protection against attack or severity of the disease will be of any long duration or of substantial degree where the area of vesiculation has been insignificant. Like other speakers, I have noted accounts in earlier writers of epidemics in this country which have apparently been mild in type, of low infectivity, and not specially fatal to unprotected infants. This type seems now again prevalent in various parts of the world, but I should not venture to ascribe it to the effect of our administrative measures, or to conclude that, as suggested in one of the papers, the type results from the organism and the host trying to accommodate one another and live together. The countries in which this type has lately appeared must vary greatly in the administrative measures applied, and in the length of time that they have had to operate in. I see nothing in the Sydney reports or in the present occurrence of this milder type of small-pox to give any security that it will not, in certain countries at least, be followed or accompanied by new epidemics of a more virulent type, one which is more severe to the unvaccinated, and more infective. The occurrences at Sydney deserve notice in connexion with the thesis of Dr. Millard that the promotion of systematic vaccination of infants is undesirable as it leads to mild and unrecognised cases of small-pox amongst adults when

infection has been introduced. Such force as there is in this argument seems to be lessened by the experience in Sydney that mild small-pox, easy to pass unrecognised, so frequently occurred among persons who were wholly unprotected by vaccination.

Dr. C. K. MILLARD wrote: I have read Dr. Armstrong's paper with much interest and appreciation. It would appear from the account given that, but for the extreme mildness of the epidemic the clinical symptoms differed but little from those usually present in mild outbreaks of small-pox, and it is a little surprising that so much doubt should have been felt at first as to the identity of the disease. Possibly the fact that the cases differed so greatly from the clinical picture of small-pox so often found in text-books, and usually depicting a severe form of the disease, may account for the apparent reluctance of the profession to diagnose small-pox at the outset of the epidemic. Probably, too, the belief, also inculcated by many text-books, that small-pox in unvaccinated persons is almost invariably very severe may have been somewhat of a stumbling-block. It has been so often prophesied that when small-pox overtakes a community which has neglected infantile vaccination the disease will inevitably decimate it, that the experience of Sydney is certainly rather surprising. One thousand and seventy cases of small-pox in a practically unvaccinated community with only one death is surely almost a record, and quite eclipses the experience of unvaccinated Leicester in the epidemic of 1904, when 321 cases occurred with four deaths. As regards the steps taken to prevent the spread of the disease, these appear to have been remarkably successful, especially when we consider the extent to which the infection had been disseminated before the nature of the disease was recognised, and the difficulty which must have been experienced in getting hold of many of the very slight cases. The way in which the epidemic was stamped out seems to indicate that when modern measures of control, including, of course, vaccination of contacts, are promptly and energetically carried out the task of controlling small-pox, even in an unvaccinated community, is by no means so impossible as has sometimes been represented. It is permissible to ask whether the results would have been so very much better if infantile vaccination had been systematically practised in Australia? It is quite certain that infantile vaccination alone, however thoroughly it be carried out, will not suffice to prevent widespread and fatal epidemics, as witness the case of Sheffield, Warrington, and Middlesbrough, all very well vaccinated towns. Dr. Armstrong says [in his "Conclusions," p. 18]: "In a well-vaccinated and re-vaccinated community the disease cannot spread." I quite admit that in a sufficiently re-vaccinated community this is true, but can Dr. Armstrong point to a single well re-vaccinated Anglo-Saxon community, past or present? If not, such a community is rather hypothetical, and I doubt if there are many who still regard universal re-vaccination as any longer a question of practical politics. Dr. Armstrong's paper is of exceptional interest, and it is satisfactory to have the facts of the Sydney epidemic put authoritatively on record.

Section of Epidemiology and State Medicine.

February 26, 1915.

Dr. Charles J. Martin, F.R.S., President of the Section, in the Chair.

SPECIAL DISCUSSION ON THE EPIDEMIOLOGY OF CEREBROSPINAL MENINGITIS.

Opened by Sir William Osler, Bt., M.D., F.R.S.

Mr. President, Ladies and Gentlemen,—I hope this discussion may allay the growing apprehension in the minds of the public and help to stimulate among the profession an interest in one of the most remarkable of epidemic diseases. As it is impossible to deal with all the features, I shall limit my remarks to the epidemic prevalence, the various strains of the germ, the sources of infection, and the serum therapy. I shall not deal with the clinical features of the present outbreak, as I do not know that there is anything calling for special remark. I have seen about twenty cases, among which the various types of the disease were represented, resembling very much the features of the disease I have seen in the United States; but the high rate of mortality shows that the type is of considerable intensity.

(1) Epidemic Prevalence.

There is nothing to add to the story of the disease in Hirsch's great work on "Geographical Pathology," which concludes with the fourth period of epidemic prevalence, about 1882. A fifth period began early in the nineties. In the United States and Canada little or nothing had been heard of the disease since the outbreak in the early seventies. Isolated cases occurred, and I remember on several occasions in Montreal the diagnosis was raised between fulminant meningitis and malignant small-pox. The wave of ascent began in 1893 with a sharp outbreak in New York. Early that winter we heard rumours of a mysterious disease in the mining districts in the western part of the

State of Maryland, and at the request of the health authorities we sent two members of the staff of the Johns Hopkins Hospital, Dr. Flexner and Dr. Barker, to study what turned out to be cerebrospinal fever. This marked the beginning of Dr. Flexner's important researches on the disease.

The disease did not spread from the mining districts of the State, and yet that winter we noticed, for the first time in the history of the Hospital, an increase in the cases of sporadic meningitis. A remarkable house outbreak occurred in the city of five cases in one family. The introduction of Quincke's lumbar puncture gave an immense stimulus to the differentiation of the forms of meningitis, and at my clinic we became greatly interested in the pneumococcal, typhoid, tuberculous, and streptococcal forms. At this period there were but few outbreaks, that in New York having been the most serious. Then in 1897-98 the disease reappeared in different localities throughout the United States, and the first opportunity we had offered to study the outbreak at first hand occurred in Baltimore. For the past twenty years the disease has had a wider prevalence than perhaps at any period since our first knowledge of it in 1805. The best indications of its progress may be obtained from the volumes of the *Index Medicus*, in which under "Meningitis" the years of prevalence are indicated by the great increase in the literature. In 1893 and 1894 the references are very scanty, and those chiefly in the United States. In 1895-96 there were references in only three of the twelve numbers. In 1896-97 the reports were chiefly from Germany and Italy. In 1898 the references are more numerous, particularly in the United States, and this year was marked by the elaborate study of Councilman, Mallory, and Wright. During 1899 and 1900 there were many outbreaks in France, and a few in the United States. For the next few years the literature is not so full, but since 1904 there has been a progressive increase in the references under cerebrospinal fever, the numbers of which often exceed those of pneumonia. New York suffered severely in 1904-05, Silesia from 1905 to 1907, France in 1908-10, and the south-western States of America in 1911.

We are still in the fifth period, following Hirsch's arrangement, as since 1893 there has not been a break sufficient to constitute the epidemics of the present century as a separate cycle. A feature of special interest has been the serious prevalence of the disease for the first time in the United Kingdom. To 1907-08 there had never been an epidemic of any extent. The outbreaks noted by Ormerod in Allbutt

and Rolleston's " System of Medicine " show only a few scattered cases, and even the Dublin outbreaks in the early seventies were not extensive. In 1907-08 the disease prevailed in Scotland and Ireland. In Glasgow there were 1,238 cases with 838 deaths, and in Belfast 725 cases with 548 deaths. Many small epidemics occurred in other parts of the country. The public authorities became aroused, and in 1912 the disease became notifiable for the entire country. A few outbreaks of no moment have occurred during the past three years, but the Local Government Board reports show a progressive but slight increase in the number of cases—272 in 1912, 304 in 1913, and 310 in 1914. This winter, with the concentration of soldiers in camps and the unusually bad weather, outbreaks have occurred in numerous places.

The disease is following its usual course. It is never pandemic, in the sense in which we use that term of influenza. It may arise and remain in an isolated district, or there may be simultaneous outbreaks in adjacent regions, or in districts far apart. When the disease first occurred it was thought possible that over-sea troops were responsible for its importation. No doubt they are for the spread of the disease to the civilian population in the neighbourhood of the camps, but we must remember that there has been of late a rising morbidity. There was a small outbreak in 1913-14 in a western county, but the outbreaks reported to date are in very widely distant places all over the islands, and everywhere this winter conditions have prevailed which are known to be favourable to the prevalence of the disease.

The morbidity rate is one of the lowest known among acute infections; in large cities rarely more than one or two per 10,000 of the population, increasing in small places to six or even eight. As an infection it is one of the most fatal, almost equalling cholera and plague in its mortality; and as a disease it is one of the most acutely fatal, as death may occur within from ten to twenty hours. On the other hand, there are cases which rival in chronicity Malta fever. As a killing disease throughout the population it is insignificant in comparison with pneumonia. It has always been a soldier's disease, though not in these islands, but in France, Italy, and in Germany the majority of the outbreaks have been in barracks—69 per cent. of those in France. As a meningitis it has two outstanding features—the dominance of the spinal involvement and symptoms, in which it contrasts strongly with the forms due to the pneumococcus, streptococcus, and tubercle bacillus; and secondly, it is the only variety from which we see recovery, occurring in from 25 to 50 per cent. of the cases.

In epidemic features cerebrospinal fever closely resembles pneumonia—the constant occurrence of sporadic cases; the occurrence of small foci in single houses, in barracks, or in gaols; the seasonal prevalence; the mild grade of contagiousness; the transmission by carriers; and the germ has a variability as distressing to the bacteriologist as that displayed by the pneumococcus.

(2) The various Strains of the Germ.

Our knowledge of the essential ætiology began with the discovery by Weichselbaum, in 1887, of a Gram-negative diplococcus in the exudate. In 1895 the observations of Jaeger roused great interest in the subject, and the elaborate studies of Councilman, Mallory, and Wright, and those of Flexner and many French and German observers have clarified and extended our knowledge.

The meningococcus, universally admitted to be the cause of the disease, is present in the exudate, and very often in the nasopharynx of patients, occurs in the blood in a considerable proportion of cases, and is met with in the internal lesions and in the joints. It presents specific immunity reactions, agglutination, complement deviation, and opsonisation. Unfortunately, like the pneumococcus, there are several strains, with well-marked biological differences. Already important studies have been made on a variety known as the parameningococcus, which may be culturally indistinguishable, but presents serological differences as regards agglutination, &c. We shall expect new light on these points from the researches undertaken during the present epidemic, and we are particularly fortunate to have them in charge of such experts as Dr. Gordon and Dr. Arkwright, who have already devoted much study to the variety of the germ.

(3) Sources of Infection.

One thing seems clear, that the germ is of low virulence, and the disease has about the same measure of infectivity as pneumonia. As a rule, neither doctors, nurses, nor attendants are attacked. As in pneumonia, two or three cases may develop in a ward, or there may be a house epidemic of three or four cases, or the disease may break out in a gaol or in a barrack, attacking a score or more persons. We had no instance in Baltimore of infection in the hospital. On the other hand, there was a house infection in which the mother, two sisters, and two brothers were attacked. In large outbreaks, as in New York and

Glasgow, there were many instances of two or more cases in the one house. Occasionally a nurse or a doctor has contracted the disease, but this is very rare.

Two things have contributed to our knowledge of the sources of infection. In the first place, the discovery by Gee and Barlow, in 1878, of the posterior basic meningitis as a form differing from the tuberculous, the pneumonic, and the septic. It was determined by the work of Still, and of Hunter and Nuttall, that the meningococcus of Weichselbaum was also responsible for this variety. Not only in children does this sporadic form occur, but also in young adults, and it has a very variable incidence in the community. Every large hospital has had the experience such as that reported by Cecil Wall of the London Hospital in 1901, to which in the course of a few months, while meningitis was not specially prevalent in the country, twenty-one cases were admitted, nearly all in children, of whom ten recovered. So that we must accept the fact that the meningococcus is a widely spread organism in all communities as a germ of low virulence, attacking chiefly children under the age of 2, but also responsible for a certain number of cases of cerebrospinal meningitis in young adults. The 310 cases in England in 1914 represented so many individual centres, from few or any of which did the disease spread.

The second discovery of prime importance was made by Albrecht and Ghon in 1901—the presence of meningococci in the throats of healthy persons in the vicinity of cases. Already in the Boston epidemic, Councilman and his associates demonstrated them in the throats of patients. The studies made during the recent epidemics have shown how widely spread in the community are carriers. The statistics are given in the various recent manuals. The incidence of carriers among contacts in the immediate entourage of a patient has varied from 10 to 50 per cent., and they are much more frequent in overcrowded districts of a city and in barracks. The number of carriers has been found to bear some relation to the intensity of an epidemic. Another point of importance has been determined—namely, the existence of carriers who are not contacts, i.e., who have never been exposed to the disease. Upon this point we need more information. The Munich studies of Mayer and his associates found 1·73 per cent. in the garrison of 9,111 men during a period in which no epidemic was prevailing, and even a higher percentage has been found in a smaller number of cases. We have no information on this point for Great Britain, but it would be well in the near future to have studies made upon contacts in association with sporadic cases.

These recent facts enable us to explain some points of great obscurity in the transmission of the disease. The carriers, who bear a definite proportion to the cases, act as their name indicates. Healthy persons with the germs in their mouths transmit to others in the ordinary contact of intercourse—in the use of common eating utensils, in kissing, in the use of the same towels, the contact of soiled hands in handshaking, the contamination of food by carrier cooks. Just as with pneumonia, only a very small percentage of carriers are attacked, so in cerebrospinal meningitis a measure of susceptibility in the individual must be assumed. This does not explain the tendency to recur in cycles, which, after all, cerebrospinal fever shares with a great many other infections. I have quoted elsewhere Sydenham's remarks on the *genius epidemicus*. The variability of the organism lends support to the view that certain conditions may favour an increased virulence — certain occult and unaccountable changes, of which as yet we know nothing, but which may explain the cyclic recurrence, just as in the great pandemics. Increased susceptibility on the part of the patient, due to depressing influences of hard work, overcrowding, and bad weather, has also to be taken into consideration.

How the meningococcus reaches the meninges is still under discussion, whether by direct invasion of the meninges from the nasopharynx through the ethmoidal or sphenoidal sinuses, or through the blood-stream, with specific localisation on the meninges. Certainly the sudden onset, the fulminant cases, in which death may occur before there is any purulent exudate in the meninges, and the presence of the meningococcus in the blood, are points strongly in favour of a primary blood infection. The question of detection of carriers and their treatment I must leave to others.

(4) The Serum Therapy.

Meningitis caused by germs other than the meningococcus is almost uniformly fatal. Personally, I have not yet met with an instance of recovery in the pneumococcal, tuberculous, or streptococcal variety. It is the high case mortality that makes an outbreak of the disease so dreaded. Until recently our treatment has been palliative—hot baths, lumbar puncture, and sedatives. The serum treatment, for which we have largely to thank Dr. Flexner, is directed towards the destruction of the organisms themselves in the meninges, acting by bacteriolysis and phagocytosis, at the same time neutralising the poisons. One

cannot but be impressed in some cases with the remarkable results seen after the use of the serum. The statistics collected by Flexner, and the experiences of Robb in the Belfast epidemic, and the reports from the recent outbreaks in the south-western States are most favourable. To be successful it must be begun early and conducted vigorously. Others will speak of the results during the present outbreak, which have not, I am sorry to say, been very satisfactory. We are dealing apparently with refractory or fast strains, but these will probably be met by the manufacture of a serum from the germs of the present epidemic. As an outcome of the recent studies at the Rockefeller Institute by Martha Wollstein, the employment of a polyvalent serum including the parameningococcus strain is recommended. Another difficulty no doubt has been the technique, which naturally cannot always be carried out by experts; but these are points which I must leave for them to discuss.

DISCUSSION.

The PRESIDENT said he was sure all present had listened with great interest to Professor Osler's masterly and particularly stimulating introduction to the discussion.

With the permission of the meeting, and as far as the amenities of the situation allowed, he suggested that in the discussion the procedure should take somewhat the following form, which was very similar to that outlined by Professor Osler. He would call, first, upon gentlemen who could contribute to the knowledge of the characteristic features of past epidemics, from their personal experience. There were present that evening an extraordinary assemblage of gentlemen who were capable of speaking with the highest authority upon this and various other aspects of the subject, such as the bacteriological, the public health, and the administrative sides. He would then call upon gentlemen who would resume, for the benefit of the Section, present knowledge of the biological characters of the meningococcus, from the point of view of their significance epidemiologically. Next he would ask gentlemen to speak on the interpretation of the peculiarities of epidemics of cerebrospinal meningitis in the light of the knowledge at present available. And lastly sanitary measures, prophylaxis, would be discussed. The practical difficulties in dealing with the various suggestions that emerged would be considered in such detail as the time permitted.

Sir William Osler, in his address, mentioned treatment, but he (the President) pointed out that the Section was Epidemiological, and was primarily interested in prevention. No doubt the subject of treatment would intrude itself in the course of remarks, and that aspect was of great interest to all of us at the present time. He thought, however, that treatment should as far as convenient be kept in the background until towards the end of this debate.

The subject they had met to debate was so important that, as he understood the room would be available on the following Friday, at the same time, he hoped members would find it advisable to adjourn the discussion, as it seemed probable that not more than half the proposed speakers would have time to give the Section the benefit of their varied experience in the present meeting.

Dr. NEWSHOLME said he would be unable to devote his remarks to any one of the compartments which the President had plotted out for speakers, but he would like to make a few general observations. He was certain he was speaking for every member of the Section present in expressing their deep sense of indebtedness to Sir William Osler for his valuable introduction to this discussion. He was particularly glad to hear Sir William strike an optimistic note, and state that, nothwithstanding the high mortality of the disease, it was not a killing disease in the large sense of that term. He wished to add some figures to those brought forward by the opener.

Taking the experience of London during the seven weeks ending on February 20 of this year, nineteen deaths were attributed to cerebrospinal fever. For the same period sixty-nine times as many were recorded as having been due to pneumonia; nineteen times as many due to measles; six times as many as due to diphtheria; and more deaths were due to typhoid fever than to the very important disease now being discussed. With regard to notifications, in the same seven weeks in the whole of England and Wales, there were 296 cases of cerebrospinal fever notified in the civil population, and the number of military cases did not exceed that. Of other notifiable diseases, puerperal fever showed 355 cases notified, as against about 300 civil cases of cerebrospinal fever. Of enteric, over 1,100 cases were notified, and of diphtheria about 8,000. He did not mention those figures with any idea of minimising the importance of this extremely fatal disease, but in order that it might be regarded in its proper perspective in relation to other diseases. It was a satisfaction to know that cerebro-

spinal fever was not likely to be pandemic. As Sir William Osler had said, indeed, under the influence of a more open-air life than our soldiers and civilians could obtain at present it was likely to diminish, and it might be hoped that this would have happened before many weeks or months had passed. During the immediately succeeding weeks unrivalled opportunities for investigating this disease would occur, and it would be a pity if the present stress of work were allowed to prevent such study being carried out. He did not doubt that many of those present, despite their strenuous activities at present, were making notes of their cases which would become extremely valuable to epidemiologists.

It was clear, from what Sir William Osler said, that there were many problems connected with this disease which still remained unsolved. There were two, to his mind, puzzling facts. The first was, that during the present epidemic there had scarcely been an instance in which more than one case of the disease had occurred in an invaded house. The same remark applied to the case of the military huts, in which twenty to forty soldiers had been living. Against that important fact was to be set another—namely, that apparently healthy carriers of the meningococcus were able, in some instances, to infect those who came into intimate association with them. He confessed that he saw no means of complete reconcilement between those two important facts, both of which appeared to have been well established. There was need for further light on that aspect of the disease.

Another point on which more light was needed was as to the relationship between overcrowding and the existence of this disease. It was recognised that in other countries it had been a barrack disease. In this country we had not had experience of it in military barracks until the present year, at least not to any considerable extent. He therefore submitted the question whether the association between the disease and overcrowding was as close as it had been claimed to be. One knew that, on occasion, it had sprung up in village communities, in which no evidence of overcrowding had been forthcoming. He hoped this supposed connexion would be further investigated.

Similarly with regard to overwork. It seemed fairly clear that the disease attacked more commonly young recruits, who, presumably, were being trained in an exhausting manner. But was that an essential association? Or might not the disease occur in conditions in which great fatigue did not enter?

In reading through a large number of manuscript reports of outbreaks of this disease during the last few weeks, he had been much struck by the fact that, in a large number of instances, there were, for about a fortnight at the beginning of the illness, catarrhal symptoms, which afterwards merged into meningeal symptoms. It would be interesting to know whether those early symptoms were due to the *Micrococcus catarrhalis*, or whether they represented the true catarrhal stage of cerebrospinal fever. If the latter, he assumed it would be analogous to the catarrhal stage of whooping-cough, followed by the stage marked by nervous manifestations. Possibly light might be thrown on that feature by more systematic bacteriological examination of the cases of catarrh occurring under military conditions.

With regard to the preventive aspect of the disease, the profession was at present groping to some extent in the dark. One knew it was extremely valuable to have a bacteriological diagnosis of the contents of the cerebrospinal fluid, but did one gain value for the trouble by having a bacteriological examination of the nasopharyngeal secretions of contacts? He thought one did, but there were no observations on that on any considerable scale in this country, and particularly there were no observations as to the frequency with which the meningococcus could be found in persons who, apparently, were not contacts. Until more information was forthcoming on this point one could not assess at its true value the importance of the taking and the examination of swabs from contacts. He hoped that such observations would be available before long, and he would be glad if, in that evening's discussion, some light were thrown on some of the points he had mentioned.

Again, he wished to thank Sir William Osler for his valuable and suggestive address.

Dr. CHALMERS (Medical Officer of Health, Glasgow) said he desired to approach the subject entirely from the epidemiological standpoint and to base his remarks mainly on the Glasgow outbreak of 1906-07, although the outstanding features of that outbreak had already been dealt with in a previous paper.[1] No new facts concerning the outbreak had emerged, but it was possible now to get some of them into better perspective.

The epidemic might be said to have been self-limited. Like all true

[1] "Some Recent Manifestations of Cerebrospinal Fever," *Trans. Epid. Soc.*, 1906-07, n.s., xxvi., pp. 121-142.

epidemics it came and went, and although we isolated cases and contacts, and douched the nasopharynx of those who had the meningococcus therein, he doubted whether it could be said to have influenced the course of the outbreak. The great majority were single cases in separate households. Many deaths occurred at home before removal could be accomplished, but it could not be said that there were appreciably more secondary cases associated with them than occurred in association with those who had been promptly removed to hospital.

While actively engaged with the first phase of the epidemic the outbreak suddenly acquired a fulminant character (January, 1907); and in the following June the numbers occurring monthly fell almost by one-half without any change in administrative measures having occurred to explain the fall.

With regard to temperature and the presence of conditions favourable to the development of catarrhal affections, it might be said that, while the mean temperature of the autumn and early winter months of 1906 was above the average for that period of the year and the rainfall was greater, the following months, which saw the development of the fulminant period had—save January and March—a monthly mean temperature much below the average and conditions generally which were favourable to catarrhal states.

With regard to the features determining the outbreak, it might be suggested that germinal activity played a dominant part. The view gained some support, he thought, in certain features of the fulminant period: (*a*) Clinically the cases were more severe in type, death in many instances occurring after a few hours; (*b*) multiple cases in households were more frequent; (*c*) there was a tendency for the disease to invade families living under more comfortable social conditions; (*d*) a change in the age-incidence, especially in the reduction of the proportion of children aged under 1 year; this feature, indeed, was to some extent similar to what had occurred in the age-distribution of diphtheria prevalence in recent years.

With regard to the diseases more frequently mistaken for cerebro-spinal fever, he found that in the early period of the outbreak 8 per cent. of those notified were tubercular meningitis, and that, as the disease became less frequent in 1908, the proportion of tubercular meningitis rose to 16. Pneumonia, on the other hand, supplied in the two periods 3 and 8 per cent. respectively.

With these preliminary notes he now proposed to ask whether there was anything in the distribution of these cases which brought it into line

with that of other infectious diseases? In other words, could we learn anything of the laws which determine the spread of cerebrospinal fever by comparing its distribution and movement with that of other infectious diseases? The Glasgow experience might be summarised in this way: (*a*) The disease became grafted on the volume of meningeal disease in the spring of 1906; (*b*) it assumed a recognised prevalence in the latter half of that year, over 150 cases being notified between July and December; (*c*) it required greatly increased prevalence early in 1907, reaching its maximum—184 cases—in February; (*d*) in June following it underwent a sudden decrease, and the cases notified in December fell to eighteen; (*e*) its subsequent history may be illustrated by the cases occurring during the last five years, which reached a maximum of bacteriologically verified cases in 1911 of thirty-eight, while the following year had the lowest number recorded—namely, six, all of whom died.

It might, therefore, be that the few cases now occurring annually brought the disease to the level of the prevalence existing before its epidemic outbreak in 1906-07. The features of an epidemic wave were, therefore, fairly complete. There was an unobtrusive beginning, a period of persistent but restricted prevalence, then the major movement of the epidemic, followed by its decline.

Question of Traceable Association among Cases.

It would be useful at this point to consider the available information regarding the distribution of recognised cases when our outbreak began. It was recognised in several centres almost simultaneously. In March, 1906, two children of one family resident in Glasgow sickened with a considerable interval between them. In the following month three children in another family which had moved into Glasgow from the surrounding district fell ill. Several cases also were recognised in the general infirmaries—one from so far north as Bonar Bridge, others from Lanarkshire and the County of Renfrew.

As these relatively few cases had a wide distribution it was not necessary to assume that the disease had reached them from over-seas. Indeed, in the paper already referred to he had drawn attention to certain cases which had been recognised in the practice of one practitioner in Glasgow since 1891. Very soon it became obvious that there was a lack of traceable association between invaded families. In pre-epidemic small-pox a large proportion of the cases occurring were traceably associated, so also in measles and typhus fever. During their

last three years' experience of typhus fever there were ninety-two cases, and no fewer than forty-nine of them occurred among known contacts with forty-three primary cases; but of the 186 households invaded by cerebrospinal fever during the first period seven only, or less than 4 per cent., had more than one case, although this proportion rose to 7½ in the spring of 1907, when the outbreak was fully developed. Moreover, outside the area of invaded households only eleven examples occurred, in which an association with cases formerly known could be established, and in these secondary (?) cases the interval varied from a few hours to fourteen days.

Epidemiologically, however, the parallel with diphtheria seemed closer. By reference to some recent experience, for example, he found that of 114 households invaded by diphtheria during the month of December last, seven only had more than one case which represented rather less than 6 per cent. of the total. This proportion fitted midway between the distribution of cerebrospinal fever during its first and second period, where multiple cases in households occurred in rather less than 4 and 8 per cent. respectively.

He (Dr. Chalmers) was therefore disposed to think that, when compared with typhus fever, measles, and small-pox, the organism of cerebrospinal fever was of low initial infecting power. It might be said that this lasted so long as the conditions were favourable to the individual, but, on the other hand, when the conditions were favourable to the organism its power to infect and to kill recalled somewhat the malignancy of typhus fever and plague. In other words, its virulence would appear to become reinforced during a period such as he had called the "first phase" of their own epidemic. So, at least, he was disposed to read the contrast between their first and second phases.

One was thus led to ask, what were the conditions favourable to its spread? Variations in virulence in the organism itself must obviously be left out of account as being, for the moment at least, matter of conjecture. In the report of their 1907 outbreak he related some effort to distinguish between the history of the disease in grossly dirty houses and the more elusive form of dirt, which occurred as respiratory impurity and resulted from overcrowding. The conclusion was that the latter was the more closely related both to invasion and multiple cases. Now, when it was possible to look at it from a distance, one found a very close parallel between the attack-rate of cerebrospinal fever and the general death-rate, which might be stated in this way, that in all the wards or districts of the city, thirteen in number — which had a

higher death-rate than the average for the whole city—the case-rate of cerebrospinal fever was also uniformly above the average, as was also, with two exceptions, the death-rate; while the morbidity rate, as was probably to be expected, varied to a greater extent, and in about half of the worst areas was below the average.[1]

GLASGOW, 1909-12.—CEREBROSPINAL FEVER.

Calculated Death-rate in Population with Uniform Age and Sex Distribution.
(Males, 48,605; Females, 51,395.)

	Death-rate per 100,000.
One apartment	5·37
Two apartments	3·98
Three apartments	2·65
Four apartments	2·61
Institutions and shipping	5·13

The parallelism between insanitary conditions and cerebrospinal fever was, therefore, as close as between insanitary conditions and the causes of death generally, but it would seem that the particular form of insanitary condition which determined the parallel was that of respiratory impurity. The close relationship between respiratory disease and insanitary conditions might also be held as explaining the almost invariable experience—there were, indeed, only four exceptions—that in whatever areas pneumonia deaths occurred in excess, these also showed an excess prevalence of cerebrospinal fever. There was another feature bearing on the epidemiology of the disease, in the grouping of cases. They had seen that, although secondary cases occurred, contact groupings were rare; on the other hand, place groupings could be shown on a spot map. Further, there was little evidence that the disease spread rapidly among neighbours, because in over 1,000 cases occurring during the 1906-07 period, in twenty-four instances only had any single tenement of houses more than one household invaded. This notwithstanding that in over 300 contacts, from whom swabbings from the nasopharynx were carried out, 26 per cent. were returned as "positive." The age distribution of these carriers must be borne in mind in relation to the lessened activity of post-nasal adenoid tissue in adults, but it was certainly somewhat remarkable that in their experience anyone recognised as a carrier—that is, having the meningococcus in

[1] The following figures referred to the disease as occurring in 1909-12, and not, therefore, as influenced by epidemic virulence. They were based on a small number of cases, but suggested an excessive incidence on the one-apartment population of the city, when the population of each size of house was reduced to a standard of age and sex distribution.

his posterior nasal passages and not at the same time suffering clinically —did not, as matter of fact, develop the disease subsequently. Age alone would scarcely explain this.

Coming to the position of affairs now existing, there was, he thought, in association with the explosive period in Glasgow a certain substratum of low temperatures which fostered catarrhal conditions. Mainly, he thought, however, that the continuance over some months of cases not numerous but widespread might tend to give the organism increased virulence and lead to an explosion, as it did in February, 1907. It was too late, he believed, to spread the population out after a particular overcrowded house had been invaded. The practical question, how carriers were to be dealt with, resolved itself very much into a question of getting quit of the meningococcus. If the absence of subsequent disease in carriers did cause some hesitation in accepting the variety of organism found post-nasally and entirely unassociated with clinical symptoms as being the true organism of the disease, he doubted at the moment whether this was sufficiently strong to warrant the discarding of nasal douching; and, for his own part, he believed that quite as rapid results had been obtained in free douching of the nose with slightly alkaline lotions as with any special disinfectant. He meant not nasal syringing only but douching.

In conclusion, he was disposed to approach cerebrospinal fever administratively on the basis of a typhus fever outbreak—that was to say, virulence grew up under overcrowding—but for executive purposes diphtheria seemed to afford the closest parallel.

Dr. GARDNER ROBB (Belfast) thanked the Section for giving him the opportunity of being present, as he was greatly interested in cerebrospinal fever, as every person must be who had had anything like the experience which Belfast afforded in 1907-08, especially if they had been in any way responsible for the treatment of these cases. The Belfast visitation started at about this time of year. The remark had been made to-night that there had not been many deaths from the disease in the present outbreak, but it must be remembered that we were now only at the season when it commenced. He believed the disease was fairly general all over the three countries, for during the last three weeks he had had applications for supplies of Flexner's serum from many districts not only of Ireland but also of this country. In the extensive outbreak in Belfast, the first cases came into hospital at the end of December, 1906. Then there was a month of complete

rest, and a real start was made at the end of January. The first admissions were five members of one family, who were taken ill within thirty hours of each other. Then there were no more admissions to hospital for a month. (Up to that time he had seen only two or three cases in the small epidemic in Dublin; there had been forty or fifty cases in that city some five years before.) After the first five there were five more. Again all of one family in a separate part of the town, 2 miles away. These last five children were taken ill within ten hours of each other, and within thirty hours four of them were dead. The fifth child had a mild attack from which it recovered.

Sir William Osler had expressed the hope that good results might be expected in regard to ascertaining what were the different strains, in various parts of the country, now that there were so many opportunities for study. There were one or two points which he (the speaker) thought should be settled fairly definitely. He was particularly interested in the position of posterior basic meningitis in relation to the epidemic variety, the organisms of which were indistinguishable by cultural methods. There were very few cases of posterior basic meningitis in the north of Ireland, but the few which were seen gave a quite different agglutination; the serum from the posterior basic cases did not agglutinate the meningococcus from the epidemic cases, and vice versa. Also the posterior basic form failed to give the high opsonic index found in the epidemic variety. Houston and Rankin worked this out very thoroughly, and they obtained cocci from cases in several different localities, including Great Ormond Street Hospital, also specimens of blood from some of those cases. It was found in Belfast that the serum from these cases did not agglutinate the meningococcus from the epidemic variety, and vice versa. When using the serum in treatment, the few cases there were of posterior basic meningitis failed to respond to Flexner's serum. He had not had the opportunity of using more recently prepared sera in posterior basic cases; but possibly now that endeavours had been made to collect various strains from widely separated localities, perhaps good results were obtained from this polyvalent serum in posterior basic cases.

It would also be interesting to know how their cases compared with the French cases. Dopter found that there was one definite strain, which he called a parameningococcus, which did not respond to the polyvalent serum made in America. In France they were now combining the sera obtained from different sources, and if a case was too severe to await differentiation both kinds were immediately injected.

In the Belfast epidemic, just as in that of Glasgow, there were some extraordinary house infections. In several instances there occurred multiple cases in one house *at one time;* there were not many instances in which one case occurred in a house, and then at a reasonable period another, which might be assumed to have originated from the first. There were either several cases occurring at once, or a certain house got a name as being infected with cerebrospinal fever, and cases occurred in it at *long* intervals. For instance, one case was admitted from a house in a district where there had previously been no cerebrospinal fever; that one case was admitted into hospital in March; it recovered, and was discharged in May. No more cases occurred in that district until October, seven months after the occurrence of the first case, when there were two more cases *in that same house;* it was one of a row of small houses in a long street. These last two cases from this house were taken ill on the same day. One, which did not come to hospital and did not have serum, died in three days. The other had the serum in hospital, and made a good recovery. Again, a child was taken ill in a country district, 15 miles from the town, and there had been no previous cases there; neither were there any in that district for months afterwards. The nature of the disease was not there at once recognised; the patient was a school teacher's sister, and the school teacher went home from Belfast and nursed the child through its attack. After four days the patient died, and the teacher returned to town in September. The following Christmas she returned and spent a week in the place, which was her home. On the day following her return to Belfast she developed a severe attack of the disease, though there had been none in the district in which she resided in town. It seemed, therefore, that she had contracted the disease while spending Christmas in the house in which her sister was attacked over three months before. There were several other peculiar instances of the same kind.

As had been the experience elsewhere, no official contracted the disease—no member of the sanitary staff, and no nurses, even though the epidemic taxed the nurses and staff more than he had ever known before. Sometimes more than half the patients in a ward had to be restrained and were nasally fed, and at one time the patients were dying at the rate of twenty per week. Therefore the risk of working in association with these patients did not seem to be very great. Another peculiar point was that practically all the nurses were drawn from the local population, and when on leave they mixed freely with their relatives, and yet amongst their friends there did not seem to have been

any cases at all. There were no instances traced of the disease having been conveyed out of the hospital, and none of the nurses nor other officials in close contact with the patients appeared to have acted as carriers.

Though an embargo had been placed on the discussion of treatment, it was a subject in which he was much interested; he did not feel that prevention was his sphere. With the President's permission he desired to refer to the absolute and complete revolution which was produced by Flexner's serum. He had no personal experience of any other kind of serum injected into the spinal canal. In some seventy cases he used by hypodermic injection various German sera, which were greatly lauded, and representatives even came over and persuaded the authorities to buy large quantities of these sera, quoting good statistics from other places. But in the seventy cases, though in some he pushed the dosage and frequency beyond that laid down for hypodermic injection, he did not find it of the slightest use; in fact, the death-rate in those cases was rather higher than in the generality. In Belfast during the first eight months of the epidemic there were in hospital 275 cases with a death-rate of 72 per cent.; and anything more depressing than the work there among these cases it was impossible to imagine. They had come to the conclusion that they could do practically nothing to check the death-rate, though occasionally good nursing and frequent lumbar puncture for drainage purposes seemed to turn the scale in the patient's favour. But he thought the number of cases thus benefited was very small. In practically every epidemic the death-rate had been between 70 and 80 per cent., though a smaller rate had been shown in smaller visitations.

On September 1, 1907, they began at Belfast the intraspinal injection of Flexner's serum. In the three months immediately before starting this forty-five cases were admitted, and thirty-seven of them died, giving a mortality of 82 per cent. After the new injections were started the results were so striking that he felt afraid to publish the cases, or to say what he thought of the treatment. It not only produced a complete revolution in the death-rate, but it altered the whole appearance of the wards set apart for cases of the disease. In the first thirty cases they treated with the serum eight died and twenty-two recovered—a death-rate of 26 per cent. At that date only 50 per cent. of the cases in Belfast were being sent into hospital; friends of patients had understood that it was not a very infectious disease, and it was reported that medical men could do little for it; hence there was a

tendency to keep patients at home. During the four months that the thirty cases were treated in hospital, thirty-four were under treatment at their own homes, *not* with the serum, and of those thirty-four, twenty-nine died, giving a death-rate of 85 per cent.

Since the use of the serum was commenced there had been 117 cases admitted to his hospitals, with twenty-eight deaths — a case mortality of less than 24 per cent. Every case admitted since had been treated with the serum. *All* are included in these figures, even when death took place within thirty-five minutes of first injection. He was satisfied that this would be improved upon when the recent knowledge as to dosage and methods had been applied generally. And since the serum was used the "chronic" cases had disappeared from the wards. In the earlier series, cases were quite frequently of several months' duration, often lying unconscious and being kept alive by nasal feeding for weeks, only to die of progressive emaciation; some such cases lasted up to four months. Now that the serum was being used it almost always happened that if the patient did not die in the first fierce onset he was well in a fortnight, often much earlier. Since the use of Flexner's serum was begun "chronic" cases had been no longer met with. The same kind of occurrence was noted in New York; Dr. Holt collected records of 350 cases which had recovered before the use of the serum, and the average period of illness of those cases was a long one; in 50 per cent. the cases had shown acute symptoms for five weeks, but such cases were not seen later in New York.

The disease had been referred to that night as one with a very high case mortality rate; but he thought it was scarcely correct to say that it had a high mortality now. In the case of soldiers, they were at the best age and in the best condition for recovery. In all the epidemics before the use of serum was begun the recovery-rate improved as the young adult age was reached; in young children the recovery-rate was not so good. In an adult the diagnosis could be reached much earlier than in the case of children. Infants, aged 1 year, he found were often left for two or three days before being sent into hospital.

In Flexner's first 400 cases, which included seventy Belfast cases, that authority found that if the serum were used between the first and third days of the illness the death-rate was only 16 per cent.; if given between the fourth and seventh days it was 23 per cent.; but if its administration were delayed until later than the seventh day it was 35 per cent.

In the case of young and vigorous troops, in whom diagnosis could

be made early, he was satisfied the death-rate could be kept low—i.e., in the neighbourhood of 10 per cent. But there would *always* be a considerable number of cases in which nothing would do any good. There had been, within the past fortnight, two or three cases in a camp 25 miles from Belfast. In one of these the men had been on a route march, and came in at 5 o'clock. He went to his supper as usual, and then suddenly felt very ill, so went off to his hut. He awoke his companions at 4 a.m., moaning. He was seen by the medical officer, placed in a motor ambulance, and sent to the hospital, but he died on the way. There were instances in the large Belfast outbreak in which death ensued within nine hours of attack. In such acute cases the difficulty was to get at the men soon enough, and even if that were possible, the result would probably be the same.

He had had no experience of any serum but Flexner's, which was now supplied to Belfast by the Public Health Department, New York. That of Dopter, of Paris, prepared on Flexner's lines, had also given excellent results. Many other places were supplying anti-meningitis sera, and they might be good, but he thought it a pity to experiment with other varieties when there were sera available of such proved worth as Flexner's and Dopter's, and especially as they seemed to meet the case of a variety of strains.

Captain Ellis said that during the past few months he had been associated with an epidemic of low incidence but high virulence. It was of low incidence, in that in a body of troops of 30,000 there had been only some 40 cases in six months. Its high virulence was shown in the fact that despite treatment by serum the mortality was between 60 and 70 per cent. The low incidence was especially striking, in that the men were in crowded quarters, and were living under very arduous conditions of work and weather. The virulence of the epidemic was evidenced by the number of fulminant cases, which during the last three months had been as high as 30 per cent.; by fulminant, he meant those in which death supervened from thirty-six to forty-eight hours after the onset of the illness. Such figures, in spite of early diagnosis and persistent serum treatment, were impressive.

From Dr. Robb's and Flexner's figures one knew how low the mortality could be in cases well treated with a serum which was active against the strain of organism prevalent in the epidemic. Such good results had also been achieved by many men in many places and in different countries, the mortality varying between 25 and 35 per cent.

The high mortality, 60 to 70 per cent., occurring in the recent epidemic seemed, therefore, to show that they were dealing with a strain of organism which was not susceptible to any of the sera which had been used in the treatment of these patients. Yet during this epidemic they had been using four different sera—two American, two English. From one of the American supplies there seemed to be occasionally some suggestion of improvement, but on the whole it could be stated that serum treatment in these cases had been of no avail, and that the cases not treated with serum had done as well as those which were so treated. The agglutination experiments carried on were confirmatory of this idea. With the two English sera which they had used there was no agglutination of the strains of meningococcus isolated from the cases. With the two American sera there was only slight agglutination, that is, agglutination in a dilution of 1 in 40: agglutination in such dilution is of no significance.

Two possible explanations of this failure to agglutinate immediately suggested themselves: (1) That the organism concerned belonged to the group of inagglutinable meningococci which has been recognised by all workers with this organism; (2) that the strain concerned belonged to a group which had not been included in the production of any of the sera used and that the failure to produce agglutination represented a true failure of antibacterial action. That the first of these was not the explanation was strongly suggested by the fact that the serum from one of the convalescent cases gave good agglutination in a dilution of 1 in 320.

It seemed probable, therefore, that they were dealing with an organism which was insusceptible to the action of any of the available sera. This idea was strongly supported by the results obtained by the use of a new serum prepared at the Lister Institute, in the preparation of which strains isolated from patients in the prevailing epidemic had been used. He hesitated to comment on this, because the opportunities to use it had as yet been so few. Of the three cases in which it was used, two were moribund when the serum was given, and died two or three hours after the injection, but the other case had been so striking that the result seemed to merit comment. This patient was taken ill on November 25, and treatment was started within forty-eight hours, but without noticeable benefit; all four sera were employed. On January 6, their laboratory having been established so that they were in a position to make cultures, cultures were made, for the first time, from the spinal fluid, and a positive result obtained. The patient had

at this time received twenty-one injections of serum. Treatment was then discontinued. Patient's condition remained unchanged, the temperature varying between 100° F. and 102° F. until February 11. On the latter date they obtained from the Lister Institute serum from a horse which had previously been highly immunised with numerous strains of meningococcus—a polyvalent serum. To this horse had been added the organism isolated from three patients in this epidemic, including this particular patient. On February 11, 30 c.c. of this serum were given. From 1 c.c. of the fluid removed at this time fifty colonies of meningococci grew. On the following day, fluid removed at puncture gave only three colonies; 30 c.c. of serum were again injected. Next day the treatment was omitted. On the following day a further injection of 30 c.c. was given, and the fluid removed was on this occasion sterile. Clinical improvement set in immediately the new treatment was commenced, and in four days the temperature had reverted to the normal, and had now remained so for ten days. A decubitus had commenced to heal before, but now the healing became rapid, and the patient promised to make an uninterrupted recovery. He thought that the experience which he had related was gratifying, in that it suggested that even when the outbreak did not yield to stock sera, one could in a few weeks produce a serum from the organism isolated from the cases from which efficient results could be expected.

Dr. NEWTON said that, in his district, since January 25, twenty cases had been notified, among a normal population of 18,000 plus a military population of 6,000. Only four of the cases had occurred in civilians. Ten of the twenty cases succumbed. There had been only one case of known definite contact, in the person of an R.A.M.C. orderly. The cases occurred in three wards of this district—three in one ward, one in another, and all the others in the third ward. All the cases had been carefully isolated, and had been treated by sera, but he was not sure whether the Flexner serum was included.

Dr. COLEBROOK reminded the meeting that the President referred to the question of prevention, but that aspect had scarcely been touched upon since, even by Sir William Osler. But Dr. Robb pointed out how excellent a time was the present for investigating the disease, a point which was well worth pressing home. The business of this Section was to consider the prevention of epidemics. Even if it were not a "killing disease," there was an opportunity of learning something

which would prevent serious epidemics of it in the future. One of the main problems to attack was that of carriers. It seemed to be a well-established fact, in America as elsewhere, that for every case of the disease with symptoms there were from ten to twenty carriers. During the last few weeks he had gone through the cases of all the contacts in one town in the South of England, and in his first examination there he found twenty-four people carrying the microbe in the nasopharynx, while there were only two cases of meningitis. It seemed impracticable to attempt to isolate all carriers, because by this time they were so widely distributed; but he would welcome suggestions for clearing out the microbes from the air passages. He had been busy for a fortnight on that question, studying seven people who had the organism in almost pure culture in the back of the nose. At present he had not succeeded in getting rid of the microbes. In America, spraying with all kinds of antiseptics had been found to be of no use. Apparently some had had success from spraying with anti-meningitic serum, the microbes disappearing in, on an average, ten days. Early in the present week he was told there were hopes from the use of a preparation of silver iodide. Tests *in vitro* seemed to hold out some hope when he treated nasal secretions mixed with microbes. He sprayed three of his contact cases at two-hourly intervals from the nose to the nasopharynx, and in the reverse direction, and next morning he could find no meningococci in them. He left them a day without treatment, and on the following morning he made swabs again, and was disappointed to find the microbes as numerous as ever. He took it that the experience showed that the microbes must get into crypts, and possibly also into the antrum of Highmore and the other sinuses. Dr. Sophian had tried inoculation of carriers in 1912, and in six cases he got rid of the microbe in a week after the third dose of vaccine, but he did not say how many times he failed. Still, that seemed a hopeful line to pursue, and Dr. Colebrook was now trying the treatment on three of his positive cases. If he did not achieve success with subcutaneous doses he would give the vaccine intravenously. He had used, up to the present, only small doses of vaccine, starting with 50 millions, and increasing to 200 millions; Sophian, however, had employed doses of 500 and 1,000 millions, and by these obtained satisfactory immunising responses, as judged by opsonic and complement-fixation tests.

He would be very glad if any member could help him by suggestions as to how to deal effectively with the carriers, as that seemed to be the central problem.

The PRESIDENT thought the last speaker began his remarks in a somewhat reproachful manner. The Section had not lost sight of the points raised by him, and a number of gentlemen had accepted the invitation of the Society's officers to speak in the debate on this and other aspects. It was hopeless, owing to the already late hour, to think of going into these important matters to-night, and he hoped the adjournment to a week hence would now be moved. If that were approved, next week the interpretation of the characteristic features of epidemics of cerebrospinal meningitis, so far as it was at present capable of interpretation, would be taken up. The biology of the meningococcus and the whole question of carriers, including the difficulty of finding them, would, he hoped, be adequately discussed. There were a number of gentlemen engaged in this work who were only too anxious to come together at these meetings and compare notes. If time permitted, he hoped that the question of treatment would also be dealt with. He was sorry that the time limitations prevented more being done at one meeting, especially as some members had come from considerable distances.

(On the motion of Fleet-Surgeon BASSETT-SMITH, seconded by Dr. BUCHANAN, the debate was adjourned to Friday, March 5).

Section of Epidemiology and State Medicine.

March 5, 1915.

Dr. Charles J. Martin, F.R.S., President of the Section, in the Chair.

SPECIAL DISCUSSION ON THE EPIDEMIOLOGY OF CEREBROSPINAL MENINGITIS.[1]

The President (Dr. Charles J. Martin, F.R.S.) reminded members that they had the privilege of listening last week to Sir William Osler, who opened the debate, Dr. Chalmers and Dr. Robb, who had experience of the most important recent epidemics of the disease in Glasgow and Belfast respectively, and to Captain Ellis, who had been studying the recent outbreak amongst the Canadian contingent on Salisbury Plain. The Section was greatly indebted to these gentlemen for coming to London to give them the benefit of their experience. They had dealt with the characteristic features of epidemics of cerebrospinal meningitis, and he might briefly summarise what had been learned from them, under three heads: (1) The patient might be (though only exceptionally was) the source of infection; (2) as a rule, however, no direct relationship had been discovered between one case and another; (3) epidemics showed a winter incidence, reaching their maximum in the spring-time. He did not propose to endeavour to interpret these features, because there were a number of gentlemen present who were more worthy of being listened to on those points than he was himself.

On the present occasion he suggested that, having disposed of the characteristic features of the epidemics, the meeting should proceed to discuss, first, how far it was possible to interpret these characteristic features in the light of existing knowledge; then, what sanitary measures were indicated and how far they were practicable; lastly, if time should permit, the question of treatment could be discussed, for, though not strictly an epidemiological topic, time spent on that subject would be certainly profitable.

[1] Adjourned from February 26.

The Bacteriology of Epidemic Cerebrospinal Meningitis.

By J. C. G. Ledingham.

What I have got to say in regard to the bacteriology of epidemic cerebrospinal meningitis is doubtless familiar to many here present; but it is perhaps well, in a general discussion, to restate briefly certain points in connexion with the biology and distribution of the meningococcus which have a distinct bearing on the epidemiology of the disease.

The meningococcus of Weichselbaum is universally regarded as the essential specific cause of epidemic cerebrospinal meningitis, as also of certain forms of sporadic meningitis. It belongs to a small group of Gram-negative diplococci, the only other important member of which is the gonococcus. In many relationships, and especially the serological relationships, it bears a close family resemblance to the *gonococcus*, and if the distribution of the *gonococcus* were other than it is, there is no doubt that the difficulties in differentiation, surmountable though they are, might be greatly increased. Records of cerebrospinal meningitis, as we know it, are wanting before the year 1805, and the daring hypothesis has been put forward that the *meningococcus* may have arisen as a mutant form of the *gonococcus*, with its seat of predilection in the meninges instead of the genital tract.

It would be idle, however, to speculate further on this point at present, as research is hampered by our inability to communicate to laboratory animals either cerebrospinal meningitis or gonorrhœa, as we know them. The serological and other affinities that exist between the *meningococcus* and the *gonococcus* find an interesting parallel in those that exist between the bacillus which causes pseudo-tuberculosis in rodents and the bacillus of human plague.

The *meningococcus* is a delicate short-lived organism and requires for its successful isolation and for a variable time after isolation the presence of serum, ascitic fluid, or some such substance in the nutrient medium. Later it can grow without these additions, but it still retains certain properties which mark it off as an organism which cannot under any circumstances lead a saprophytic life outside the human body. These properties are its great susceptibility to (1) cold and (2) to drying. It will not grow below 25° C., and cultures kept at room temperature very rapidly die out. If no precautions are taken against drying the organism dies out even at 37° C. in a few days, but if such

precautions are taken it has been found possible to keep the organism alive for so long as six weeks to ten months. The observations of various authors vary somewhat, and it is unnecessary to cite details, but all are agreed that the *meningococcus* is an organism which is extremely susceptible to external influences of the kind mentioned.

When found in the lumbar fluid of cases of meningitis the *meningococcus* is readily identified by its Gram-negative character and its frequent intracellular disposition. After isolation, the growth is distinguished by its emulsibility in saline, its action on certain carbohydrates, and its behaviour towards an agglutinating serum. As organisms other than the *meningococcus* are only very rarely met with in samples of fresh lumbar fluid from cerebrospinal cases, some of these tests may legitimately be dispensed with, especially if work is pressing; but in the case of suspicious organisms isolated from the nasopharynx of cases or carriers, identification tests have necessarily to be more strict in view of the fact that other Gram-negative diplococci, such as the *Micrococcus catarrhalis* and the group of chromogenic diplococci, are liable to occur in this situation. The criteria employed for the identification of meningococci from the nasopharynx have probably not always been of a uniformly strict character, and this fact has undoubtedly to be taken into consideration in appraising the percentages obtained by various workers who have searched for carriers either during the epidemic season or at epidemic-free periods.

It would be exceedingly desirable to have an agglutinating serum which would act with some uniformity against meningococcal strains of various origin. In hunting for carriers it would be invaluable, but so far the difficulties presented by the fact that a considerable number of strains are relatively inagglutinable have not been surmounted. A serum prepared by immunisation with one strain may agglutinate only a small proportion of other strains. For example, Elser and Huntoon, in America, tested a monovalent serum against 65 strains, with the result that 25, or 40 per cent. of them, were not appreciably affected. Similar results have been got by Arkwright and other workers.

On the other hand, some of these inagglutinable strains may have slight but definite absorptive powers and may, when injected into animals, produce serums which affect agglutinable strains, though remaining without action or with only slight action on the homologous organisms.

It is possible, however, that these difficulties may yield to further research, which the present outbreak will doubtless stimulate, but it has

to be remembered that in the coccal group generally these serological difficulties due to apparent high specificity of the individual strain are constantly met with—for example, in the case of the *pneumococcus*. The phenomenon is not confined to this group, however, as very occasionally strains of *Bacillus typhosus* are met with which are relatively inagglutinable, but which are able to produce agglutinins for other strains.

With regard to the distribution of the *meningococcus*, it has never been isolated from dust, air, or fomites generally, and indeed, its short-lived character renders very remote the possibility of its transmission by such media.

Apart from the actual case of the disease, the *meningococcus* is found in the nasopharynx of a certain proportion of persons living in more or less contiguity with cases. I will refer only to its occasional presence in the nasopharynx of healthy persons at epidemic-free periods. In the book on the *carrier problem* by Arkwright and myself [1] a list of observations on this point is found, and Sophian in his book,[2] publishes a similar list. Some observers have found a small percentage of carriers in such populations, while others have found none, but the total number of cases examined has not been large. The only large series is that by Mayer and Waldmann (1910), who examined 9,111 men in the Munich garrison at an epidemic-free period and found roughly 2 per cent. of carriers amongst them. In a recent paper in the *British Medical Journal* Sir William Osler quoted these figures and added a point of exclamation, the inference to be drawn probably being that he (Sir William) was inclined to subscribe to Mayer's sweeping generalisation from his figures, namely, that the *meningococcus* was a ubiquitous organism occurring in 2 per cent. of normal people.

This question is, of course, a most important one. If one were to accept a generalisation of this kind, it would amount to placing cerebrospinal meningitis on the same plane as pneumonia, where epidemiology does not come in—that is, if we accept the current view that a person gets pneumonia by infection from a strain which has been growing saprophytically in his respiratory tract.

Now, it must be said that at the time some doubt was cast on these observations of Mayer by his own compatriots, and in a footnote to his paper Mayer refers to this feeling of scepticism; but probably the feeling was one rather of disagreement with his generalisation above referred to. In my view there is no reason to doubt the figures,

[1] Ledingham and Arkwright, "The Carrier Problem in Infectious Diseases," Lond., 1912.
[2] Sophian, "Epidemic Cerebrospinal Meningitis," Lond., 1913.

but in appraising them one has to remember that the population examined was one in which cases had been occurring yearly. It is therefore not at all improbable that some of his carriers revealed at the meningitis-free period had been chronic carriers for some months, while others had been infected by such carriers. As a matter of fact, Mayer admits that four out of thirty-five persons who were carriers during the meningitis season proved to be still harbouring the *meningococcus* at the meningitis-free period. The way in which one carrier gets surrounded by a zone of carriers has been very clearly demonstrated by Selter, whose observations are to be found in the book on the carrier problem.

The presence of such carriers at an epidemic-free period is indeed necessary to explain the peculiar manner in which cerebrospinal meningitis springs up in areas between which and previously infected areas no apparent connexion can be traced. The chronic carrier, with probably a zone of carriers round him, acts as the bridge between one epidemic and the next. A generalisation, therefore, like that propounded by Mayer is entirely unjustified at the present time, and is calculated to give a false sense of security to the general and medical public.

The Interpretation of Epidemiological Observations by the Light of Bacteriological Knowledge.

By Joseph A. Arkwright, M.D.[1]

Characteristics of Epidemics.

One of the most out-standing features of epidemics of cerebrospinal meningitis is the isolation or want of ascertained contact between the cases of the disease. This has been dwelt on and emphasised by previous speakers. The majority of houses attacked afford only one case, but the proportion varies in different epidemics. In the Texas epidemic (1912) reported by Steiner [10], out of 2,135 cases 313 (14·7 per cent.) occurred in houses with more than one case, and of 1,975 houses attacked 135 (6·9 per cent.) had more than one case. It is, however, a feature of such multiple house attacks that the cases have often occurred simultaneously in the same house, as in the striking occurrences reported by Robb [9] at Belfast, when five cases occurred almost simultaneously in one house on two separate

[1] Lister Institute, Chelsea.

occasions at the outbreak of the epidemic. Where an interval elapses between multiple cases occurring in a single house it is often considerably longer than the time which has been regarded as the incubation period by several writers—i.e., one to five days. Bolduan and Goodwin [3], in their report on the New York epidemic (1905), record thirty-four instances in which the interval between two cases in one house was observed: in fourteen instances it was one to seven days; in five instances it was one to two weeks; and in twenty-five instances it was two weeks to three months. It is almost universally accepted that doctors and hospital nurses seldom contract the disease. These and similar facts which have been observed in many epidemics are evidence against the spread of the disease being usually due to direct infection from one patient to another. In fact, the direct communicability of the disease has been called in question by some epidemiologists.

Epidemiological Hypotheses.

To explain the disconnected occurrence of cases two explanations suggest themselves:—

(1) The hypothesis that the infective material is distributed widely outside the human body and remains active sufficiently long to be conveyed long distances by external agencies. This assumption is contradicted by the known sensitiveness of the meningococcus to ordinary air temperatures and to drying, especially soon after its separation from the human body.

(2) The transport of the causal organism inside the body of human beings, who are either healthy or suffering from a mild unrecognised form of the disease, and its subsequent communication by them to other susceptible persons. Such human intermediaries have occasionally been traced by clinical observers, as in the instance quoted by Steiner and Ingraham [11] in their report on the epidemic at Hartford (Connecticut), 1904-05. A hospital attendant in charge of a case of meningitis went home, and four days later his child sickened with the disease. Other such instances have been reported by Bolduan and Goodwin [3].

Carriers.

The assumption of intermediate human carriers is amply justified by the discovery that meningococci are often present in the nose or throat of persons who have been associated with cases of meningitis,

but are themselves free from meningitic symptoms. These observations have been made by many bacteriologists in different countries — e.g., v. Lingelsheim [8] (1908) examined 264 contacts of cases and found 9·5 per cent. to be carriers; Bruns and Hohn [4] (1907) examined 609 close contacts and found 36·7 per cent. carriers; Black [2] (1912) examined 801 close contacts and found 28·3 per cent. carriers.

Carriers may or may not show obvious inflammation of the nasal or pharyngeal mucous membrane, and the proportion of those with an unhealthy nasopharynx has been variously estimated by different writers.

The duration of the carrier-state is variable, but apparently most carriers no longer harbour the meningococcus after three weeks, though some may continue to yield cultures for months. Trautmann records observations on carriers who remained infective for at least sixty-six days. A second examination after an interval of one week often gives a negative result and further examinations are equally unproductive. Of thirty carriers whom I recently observed twenty-six gave a positive result on one occasion only, though all were re-examined three times during an isolation of three weeks to a month. One gave a negative followed by two positives; one a second positive result and then two negatives; and two gave a negative on second examination, then a positive followed by another negative result. Of the contacts examined by all observers, adults have more frequently been found to be carriers than children (*vide* Bruns and Hohn [4], &c.). These persons, then, who are carrying the meningococcus in their nasopharynx can transport it for long distances and infect others who are far from any case of meningitis.

The readiness with which bacteria are given out in coughing and sneezing, and even speaking, and can in this way be transferred from the nose or throat of one individual to another are matters of common knowledge. Grosser forms of infection by kissing, or eating and drinking from utensils used without washing, give other obvious chances for transference of the meningococcus to the mouth and throat of other persons from carriers.

Portal of Entry.

Further evidence that the disease is acquired in this way is afforded by the observation that the meningococcus is present in the nasopharynx of cases of meningitis in the early stages of the disease. It has been found by many observers and with much greater frequency during the first few days after the onset of the disease than at later

stages. Goodwin examined the nasopharynx of fifty-two cases of meningitis and isolated the meningococcus from twelve out of twenty-two examined in first week, five out of fifteen examined in second week, none out of fifteen examined in third to ninth week, and one case on sixty-seventh day of disease, showing that the meningococcus may persist a long time.

Among convalescents recently under my observation one man yielded a culture on the thirty-fifth day from onset. v. Lingelsheim [8] examined the nasopharynx of forty-nine patients in the first two or three days of the disease and found that 93 per cent. gave positive results.

These observations complete the cycle of the meningococcus in passing from the nasopharynx of a carrier in the neighbourhood of one case of meningitis to the nasopharynx of a second case of meningitis and leave little doubt as to the method by which the disease is spread.

The detection of meningococci in the blood of patients affords an intelligible explanation of the route from the nasopharynx to the meninges. Elser made positive cultures from the blood of ten cases out of forty-one examined. It is probable that all those about to develop meningitis are carriers during the incubation stage, and in this way may be explained some of those instances recorded by Steiner [11], Bolduan [3], &c., in which two children of different families after playing together developed the disease within a few days, either simultaneously or at a short interval.

Bacteriology of Carriers.

As is now well known, the region of the posterior nares and the upper part of the pharynx is the chief site in the air passages of meningococcus infection. This region is best reached with a small swab on a bent wire introduced through the mouth. In the case of adults difficulty has seldom been experienced in obtaining a swabbing without contamination from the tongue or tonsils or uvula. A broad spatula and a forehead-light are usually required. The best method of inoculating culture plates was found to be the following: Two plates of serum agar were used. Only the extremity or last $\frac{1}{4}$ in. of the swab was rubbed on the culture medium at one edge of the first plate and a glass rod was used for distributing the material over the first and second plates, the rod being used in such a manner as to progressively diminish the amount of seeding on the plates. After incuba-

tion at 36° or 37° C. for twenty-four hours sufficient growth had usually taken place; occasionally plates were incubated for another twenty-four hours, but very seldom was any advantage gained. The cultures made in this way showed growth of very few organisms except pneumococcus, streptococcus, *Bacillus iflnuenzæ* (or bacilli resembling this organism), and Gram-negative cocci. Films made from a loop drawn across the first and second plates and stained by Gram's method showed whether Gram-negative cocci were present in any considerable numbers. Individual colonies of different types were then examined by stained films. The colonies of meningococcus or similar organisms could with a little practice be usually recognised, especially with the assistance of a low power microscope. Colonies of *Micrococcus catarrhalis* could usually be excluded by their cohesiveness and peculiar consistency when touched with a wire.

The micro-organisms which gave most trouble by reason of their resemblance to meningococci were Gram-negative cocci of the chromogenic group, which form more or less yellow pigment. As pointed out by Elser and Huntoon, if glucose is added to the agar the yellow colour of the growth is often more obvious. Colonies of Gram-negative cocci were picked off to serum agar and after incubation were subcultured the next day to: (1) serum agar and placed at room temperature (an incubator at 20° C. is better); (2) litmus serum glucose peptone water; (3) litmus serum cane-sugar peptone water. (The litmus must be only just neutral—i.e., not too alkaline.) The tubes containing sugars were incubated at 37° C. After forty-eight hours those strains were excluded which (1) grew at room temperature; (2) formed no acid on glucose but showed growth: or (3) formed acid from both glucose and cane-sugar.

A fallacy to be avoided is the inclusion of streptococci in the culture. This is a very easy error to fall into on account of the small size of the streptococcus colonies. If no growth took place at room temperature and acid was formed from glucose, but not from cane-sugar, and, moreover, no yellow pigment was seen in the serum agar cultures, the strain was considered one of meningococcus. It is obvious that at least five days must elapse before a positive result can be reported. No agglutination tests were performed, as no serum was obtainable which would agglutinate any large proportion of undoubted meningococcus strains from the cerebrospinal fluid of cases.

A polyvalent serum has been obtained by Continental workers (v. Lingelsheim [8], Kutscher [7], Bruns and Hohn [4], &c.) which agglutinated practically all available strains of meningococcus, but, as

shown by Elser and Huntoon [5], myself [1] and others, sera made from one or few strains while agglutinating highly some strains are inert towards other races of meningococcus. Probably agglutination with serum diluted more than 1 in 200 is of little value as a means of diagnosing the meningococcus, and controls with salt solution and normal serum are always necessary.

Widespread and Disconnected Cases of the Disease.

Referring again to the epidemiology of the disease, it has often been remarked that not only has no direct contact between cases occurred but that the cases are so isolated and widely distributed that no connexion by means of a single intermediary can be considered possible. This suggests that the infective agent after being harboured by contacts of a case is passed on to other carriers in series, one or other of whom infects a susceptible person, who then develops the disease.

The widespread character of the disease during an epidemic requires for its explanation a wide distribution of carriers. This widespread distribution of carriers has been demonstrated in many epidemics, especially in the later stages. During a recent epidemic in a body of some 30,000 soldiers forty cases of meningitis occurred in about four months. These forty cases occurred in eighteen different battalions and no two cases occurred in the same hut or tent.

In some instances in the course of epidemics the chain of intermediaries may be detected, as in the history recorded by Steiner, in which the mother of two children attended the funeral of a child dead of meningitis, and in that way must have come in contact with the relatives of the dead child. She then came home and infected her own two children, who became ill four days later, having in the meantime, in another street, whilst themselves well, infected two other children with whom they played. Steiner and others relate similar instances in which children were apparently carriers of infection before they became ill. A series of instances recorded by Jehle [6] suggests another line of infection by carriers. In this epidemic children were attacked in houses which were not connected except by the fact that the fathers worked in the same coal-pit and the same part of the pit. Jehle suggests that the damp moist air of the mine facilitated the passage of the meningococcus from one miner to another.

Widespread Distribution of Carriers.

In the outbreak mentioned above, amongst 30,000 soldiers, with which I was recently concerned, four cases of meningitis had occurred scattered through a certain battalion. The greater part of this battalion was examined for carriers. Among 887 men who were swabbed thirty-three carriers (3·7 per cent.) were found. They were widely scattered throughout the battalion. Much larger numbers of carriers have been found in certain other epidemics. In that recorded by Bruns and Hohn (1908) they not only found 36·7 per cent. carriers among close contacts of the cases, but also among 1,786 less intimate contacts they found 22·5 per cent. of carriers, and among 380 non-contacts 7·9 per cent. of carriers. The widespread character of epidemics postulates a widely spread distribution of carriers, and this is established by the above and other similar observations. The number of carriers detected has been shown in different epidemics to be twice, ten times or even forty times the number of cases of meningitis. The short time during which most carriers retain the meningococcus, and the large number of carriers which can be found at any time, emphasise the ease and rapidity with which the organisms must pass from one person to another.

Utility of Search for Carriers.

Seeing that carriers are often so widespread during the height and later stages of an epidemic it is perhaps not always of much practical utility to search for carriers at these stages. The chief conditions which make such an investigation especially desirable are: (1) means of efficiently isolating such carriers as are detected; (2) the existence of an early localised epidemic. At the same time, where means are available, the isolation of every carrier who can be discovered would probably be a gain, and the circumstances of each epidemic require special consideration.

Seasonal Prevalence.

The cause of the rise of an epidemic is not explained by the discovery of carriers, though epidemics may be transported from place to place by their means. The seasonal prevalence of cerebrospinal meningitis may perhaps be correlated with the prevalence of naso-pharyngeal affections. This corresponds in this climate with winter and in some other countries with the season of excessive dust, which has been regarded by some observers as the cause of epidemics of meningitis in parts of India, and at any rate as a contributing cause, in other hot countries.

Conception of Epidemic.

The large number and widespread distribution of carriers and the small number of cases of meningitis compared to the population forces upon one the view that for a true understanding of an epidemic of meningitis the total number of persons who harbour the meningococcus should be looked upon as constituting the true epidemic. Some of these persons are not ill in the ordinary sense of the word though they are attacked by the meningococcus, and successfully attacked in different degrees, firstly according to the amount of change produced in the nasal and pharyngeal mucous membrane; this may be considered the first stage of the disease; secondly, according to whether the meningococcus succeeds in infecting the blood, meninges, joints, &c. From this point of view—the epidemiological one—the carriers constitute the mass of the epidemic, and those who subsequently develop meningitis are only a small accidental minority. The cases of meningitis, on account of the severity and fatality of this complication, give to the epidemic its importance, and they necessarily bulk largely in the eyes of the clinician.

Whether the fewness of the cases of meningitis is due to a resistance on the part of most of the carriers, either local, i.e., in the mucous membrane, or general, in the blood or meninges; or is due to changes in the virulence of the meningococcus, there is, I believe, no definite evidence on which to come to a decision. However this may be, a consideration of all the evidence about carriers leads to the conclusion that the statement *that cases of meningitis occur when and where carriers of the meningococcus become numerous* is a more correct way of stating the facts than by saying that many carriers occur in the neighbourhood of cases of meningitis.

REFERENCES.

[1] Arkwright, J. A. *Journ. of Hyg.*, 1909, ix, p. 104; 1911, xi, p. 515.
[2] Black. Quoted by Sophian, "Epidemic Cerebrospinal Meningitis," Lond., 1913.
[3] Bolduan and Goodwin. *Med. News*, 1905, lxxxvii, pp. 1222, 1250.
[4] Bruns, H., and Hohn, J. *Klin. Jahrb.*, 1908, xviii, p. 285.
[5] Elser and Huntoon. *Journ. Med. Research*, 1909, xx, p. 373.
[6] Jehle, L. *Wien. klin. Wochenschr.*, 1907, xx, p. 8.
[7] Kutscher. *Deutsch. med. Wochenschr.*, 1906, xxxii, p. 1071.
[8] v. Lingelsheim. *Klin. Jahrb.*, xv, p. 372.
[9] Robb, Gardner. *Proc. Roy. Soc. Med.*, 1915, viii (Epid. Sect.), above, pp. 55-60.
[10] Steiner. Quoted by Sophian, "Epidemic Cerebrospinal Meningitis," Lond., 1913.
[11] Steiner, W. R., and Ingraham, C. B. *Amer. Journ. Med. Sci.*, 1908, cxxxv, pp. 351-365.

Dr. W. H. HAMER said he had been much interested in the suggestion which had been made that the meningococcus had taken origin as a mutant of the gonococcus in the year 1805. It was very useful, in epidemiology, to carry in one's mind certain dates; and he put this date of the creation of the meningococcus by the side of the date he was given in his youth as that of the creation of the world, 4004 B.C. He attached quite as much importance to the one date as to the other. One thing, however, was fairly clear, if the meningococcus was created in the year 1805, it must be admitted that that organism was unlikely to be the cause of cerebrospinal meningitis; for students of epidemiology recognised that the disease was in all probability of much earlier origin than that. There were accounts of epidemics with nervous symptoms ("encephalitis" and "febrile cerebritis") in the sixteenth, seventeenth, and eighteenth centuries. He was looking the other day at an account of an epidemic given (it was interesting to turn to such accounts) by an old London practitioner, who prefaced his remarks by saying that he was "at an advanced age and of a weakened constitution, which fairly gave him a title to spare himself the pain and labour of any intense thought and meditation"; nevertheless, he thought it worth while to put on record what he had observed about this particular outbreak. He said it "commenced in February, and the patient often had chills and flushes by turns, and frequently complained of pain in his head and joints, and generally had a cough. There was pain also in the neck and fauces, the fever was continued, and the patient might determine the fever to the brain and convert it into a frenzy. Petechiæ were apt to show themselves, and in young and sanguine subjects there might be purple blotches; if the patient were heated the perspiration would be clammy. If matters were mismanaged, the spirits were wholly thrown into confusion, an inordinate pulse set in, there was a jerking of the limbs, and death took place. Of all the fevers (this practitioner had ever seen), this attacked the brain most, and could not be detached from it without great trouble and danger." These sentences were taken from Sydenham's description of what he called the "new fever," of 1685. There was nothing there recorded with reference to Kernig's or Babinsky's signs, nor was there any mention of the meningococcus, or of the parameningococcus; yet there could be little doubt that Sydenham must have been describing the disease which they had been studying (in London, at any rate) during the past eight or ten weeks. During those weeks they had had the evidence presented by the deaths announced in the *Times*, and they had noted the excessive numbers of

people who had been recorded by the Registrar-General as dying from bronchitis and pneumonia, and as dying from influenza. There had, too, actually been a small increase in the number of deaths registered from cerebrospinal meningitis. There were the strongest reasons for believing that Sydenham's description could be very well accepted as giving a fair account of the epidemic now prevailing. The fact was, that the bacteriologists could not see the wood because of the trees. If Sydenham were back here among us and he were told that the influenza-like cases in the present epidemic were due to Pfeiffer's bacillus, and the sore throats were due to the *Bacillus catarrhalis*, and certain other conditions were due to pneumococci or to streptococci, and others to meningococci, and yet others to parameningococci, he would laugh when he realised that such crude notions were seriously entertained. He would probably tell us that the germs which the bacteriologists were demonstrating were nothing more than "associated organisms," or "secondary invaders." If we could only bring ourselves to look at the matter from that point of view we should be able to entertain broader and more philosophical notions with regard to ætiology, more rational ideas as regards prevention, and he thought, too, that there would be more hope in respect of treatment than could be entertained as long as men's minds were obsessed by the existing theories.

Professor Simpson expressed his regret at having been unable to hear the discussion at the last meeting. It might happen that some of the points he would touch on had already received attention at the meeting of last Friday.

Cerebrospinal meningitis was frequently met with in the Tropics; and within the last few years there seemed to have been a recrudescence of the disease; and he did not think the climax had by any means yet been reached. In West Africa, in 1907 and 1908, there was a very serious epidemic of it, in the northern territory of the Gold Coast —so severe that the populations of many of the villages were practically wiped out; there were thousands of deaths. Dr. Horne was sent to investigate the disease, and, notwithstanding the sceptical views just expressed by Dr. Hamer, the meningococcus was found in the patients. In a very serious epidemic which swept over a part of the highlands of East Africa in 1913, having begun at Nairobi, there were characters which proved that it could be very serious indeed. Thousands of young adults and children died. Dr. Radford, the chief sanitary officer, made an investigation, and reported that it was the most widespread and fatal

epidemic that had ever been recorded in that country, and he was a man of long experience. He (the speaker) was at Naorobi at the time, and had the advantage of going with Dr. Shircore, who was medical officer at the native hospital, and also with Dr. Ross, the bacteriologist, and seeing the work which was done there. The cerebrospinal fluid was examined in a large number of cases, and the specific micro-organism was found in 80 per cent. And in some cases it was cultivated from the blood when it was not found in the cerebrospinal fluid. A meningococcal serum was used in some of the cases, but it was found to be of no use for the natives, or for the Indians; but useful results were obtained from the employment of soamin, 5 gr. of which were injected into the buttocks once a day for two or three successive days. The results of this treatment were more satisfactory than those he saw reported in the newspapers concerning the present epidemic in England. They were not equal to those obtained in the Belfast epidemic, but the mortality was reduced to about 38 per cent. Only a few Europeans were affected, and out of eight of these treated with soamin under Dr. Gilks only one died. There was a general notion among the West African natives that the cattle and fowls of the district were affected at the same time. Dr. Horne was not able to confirm that, but it was a possibility which should be kept in view.

Dr. F. E. Batten desired to say a few words regarding the endemic form of cerebrospinal meningitis, which was always present in London. That disease was well recognised by all physicians who had to do with children. It prevailed every year, beginning, generally, in February, and reached its maximum intensity about April, falling off towards May and June. A few cases might be met with during the remaining months of the year. The degree of prevalence varied in different years. The maxima of all these years fell about April. The meningococcus obtained from these cases was of a different strain from those which occurred in the Belfast epidemic—i.e., the blood from the London cases did not agglutinate the organism from the Belfast cases, neither did the latter agglutinate the organism obtained from the London endemic cases. The cases of so-called basic meningitis, originally described by Gee and Barlow, were not, as a rule, so virulent as were those to be seen in the present epidemic. Still, at times one would see as severe a case with purpuric spots, and death in a very short period. It was an interesting problem how the disease was carried on from year to year, so that it preserved the seasonal variation to which he

had referred. The cases were treated in the general ward, and he had never seen a case of meningococcal meningitis develop in the ward. It was interesting to compare this seasonal aspect with that of poliomyelitis, for the latter showed a similar curve, but with a maximum at a different time of the year—namely, in August. In both there was the same absence of transference of the disease from patient to patient in a ward.

Mention had been made of the incubation period, and he would be greatly interested to hear facts and figures with respect to that; he had not yet been able to obtain good evidence on that point. It might possibly be so variable that it could not be established. It seemed likely, however, that the incubation period was a very short one.

He ventured to enter the almost excluded domain of treatment, because Professor Simpson mentioned that the disease in Africa had been treated with soamin. That drug was tried rather extensively in the treatment of syphilis in 1908-09, in various London hospitals, and he had seen not a few cases of optic atrophy follow the use of soamin. He therefore wished to protest against the use of that drug in the treatment of cerebrospinal meningitis. He took the opportunity of referring to the matter, especially as it was recently advocated in the *Lancet* and *British Medical Journal*.

Dr. J. Robertson said he, like so many others, had attended because he had to deal practically with the disease under consideration, and was experiencing very real difficulties. What Dr. Batten spoke of as occurring in London, he believed took place also in other large communities, and to a much greater extent than was realised at the time. Every year he had had a large number of ill-defined causes of death, and it was possible that the cause might have been this disease. Lumbar puncture and a better knowledge of the disease had led to a recognition of more cases and their proper treatment in hospital. In Birmingham he did not think a year passed without eight, ten, or twenty cases of the disease having been diagnosed. If that kind of thing was occurring all over the country, as he believed it was, as a sort of usual condition of affairs, it was necessary to search for some special causal conditions. He preferred to speak of the present visitation as an increased prevalence rather than as an epidemic. The geographical distribution was very wide: he believed there were cases in the far north of Scotland; he knew there were cases in Cornwall, and a number in the more central areas. Among troops there was now an exceptionally high prevalence, and he was at a loss to account for that;

it was the experience in other countries also where large numbers of troops were brought together.

With regard to the importance of discovering the contacts, one of his colleagues came and asked him how to examine the contacts in a certain house in which were a number of children; he had never done that, and asked how it should be done. He believed it was very difficult to get a throat swab from a child which was not highly contaminated, especially without the use of cocaine. A very practical point was whether, on one examination by swab alone, one was entitled to say a certain case was or was not a carrier. In the case of diphtheria he certainly would not base a verdict on a single negative examination.

With regard to the widespread distribution of cases of the disease, and the cause, it had been his lot to go round and see many of the troops of the new Army, and he could not help feeling that some of the conditions they had been living under predisposed, or might predispose, to the disease. His experience was that a larger proportion of cases occurred among recruits who hailed from slums than among others. He did not wish to criticise the arrangements which had been made for the men's comfort, but the men themselves in many instances, by filling up all the window crevices and shutting up the huts and billeting rooms, slept in a very vitiated atmosphere; and the bad weather might also have had something to do with the prevalence.

Lieut.-Colonel WILKINSON said that some points that he had noticed in connexion with epidemics in India might be of interest to the meeting. With regard to the possibility of lack of ventilation being a factor, in India ventilation seemed to have had no effect. The disease was most commonly met with in jails, but it prevailed in the jails which were among the best ventilated. Perhaps the worst epidemic in India was in a jail which was being constructed, so that prisoners lived practically in the open air. It had also occurred in famine camps, where the only accommodation consisted of little grass huts in which the ventilation was very thorough. But in all the epidemics large bodies of people were aggregated, and he thought that was probably the true explanation of the present epidemic. The deciding condition for an attack might be the greater congestive condition of the throat in some people as compared with that of others. That seemed to be confirmed in India, where the disease occurred in certain jails among the prisoners who were at dusty work. Other epidemics occurred in cold weather when catarrh and pneumonia were rife. The bacteriological examination showed that the cases everywhere were due to the meningococcus.

Dr. W. E. Bullock said he had had two cases which threw some light on the incubation period. One man came from St. Andrews, Fifeshire, on February 10, and had a sore throat on arrival. On February 14 he developed cerebrospinal fever. In reply to a letter, the Medical Officer of St. Andrews said there were no cases of the disease in that town. Another man came from Brighton, and developed the disease in four days. The Medical Officer of Health at Brighton, too, replied to his letter that there were cases in Brighton, but not in the neighbourhood where this man came from.

Dr. Hugh Thursfield said there was a man at the First London General Hospital who was sent up from the Guard's depôt at Caterham as a contact, and was found to have the organism in his nasopharynx. He was at the hospital a week before he developed the disease. He did not know how long the organism had been in the throat before the man came to the hospital, but contact with the disease had occurred several days before the patient was admitted to the First London General Hospital, so that presumably the incubation period was longer than in Dr. Bullock's two cases. It was easy to get pure cultures in cases of meningococcus carriers by the use of a metal tube slightly curved, inside which the sterilised swab was placed. The tube was then passed to the back of the throat and slightly rotated, so that it curved behind the palate. The swab within the tube was then pushed out into the post-palatal space, and then withdrawn, the metal tube protecting it from contact with the tonsils, &c. Of six contacts examined in this way, three were found to be carriers, one giving a practically pure culture.

Dr. Bullock: I can confirm what the previous speaker said about obtaining pure cultures.

Dr. Mitford Atkinson said the practical information desired was a knowledge of the incubation period, how to detect the disease, how to treat it, and how to deal with carriers. He thought some light could be thrown on the incubation period, for since September last some 5,000 to 8,000 Service men had been under observation at the Crystal Palace. The disease broke out among them last January, and since then some eighteen cases had occurred. From watching these it seemed clear that the incubation period was a very short one, the maximum period being probably six days; in several cases the period was less even than that. One speaker, that evening, mentioned an incubation period of a fortnight or three weeks. That was observed in typhoid fever, but not, so far as he was aware, in "spotted fever."

For the detection of this disease it is most important to know what the symptoms are. It had been said that the *meningococcus* could be detected in the blood. If that were so it was a most important point, because in *plague*, of which he had had considerable experience, the bacillus could be detected in the blood, but usually only shortly before death. The acute variety came on suddenly, with a rigor, high temperature, and symptoms of malaise. An important aid to diagnosis is the leucocytosis which is always present; this was most noticeable in the cerebrospinal fluid, and was *polymorphonuclear* as opposed to the *lymphocytosis* met with in tuberculosis. It was said that this polymorphonuclear leucocytosis was also present in the blood of patients suffering from this disease (cerebrospinal meningitis). Several methods of treatment had recently been described. Dr. Low described the treatment in an epidemic in British East Africa in 1913, by soamin —i.e., atoxyl—which was also very useful in sleeping sickness (trypanosomiasis). Of 143 cases there, 90 were so treated; and of these 56 were discharged cured, while 34 died; this gives a percentage of 62·22 of recoveries.[1] In the cases recorded by the Local Government Board as occurring in 1913 in England and Wales, there were 69·6 per cent. of deaths in the notified cases, there being 276 cases, with 192 deaths.[2]

It was very important that the disease should be detected among the carriers who were not showing symptoms. He was informed that the technique for this was a particular one. It was not sufficient just to take a swab of the throat, a special curved probe was required so that the swab could be taken well up from the posterior nares. Sporadic cases had been constantly occurring in England and Wales, and he did not think the *present* number of cases warranted the application of the term "epidemic." At any time there might be an explosion or epidemic, just as in the case of plague or any other infectious disease; indeed, it seemed not at all improbable that this might occur.

The PRESIDENT remarked that many speakers had said they had come to get suggestions as to prophylactic and sanitary measures when they had cases of the disease to deal with; but, so far, every speaker seemed to have avoided that aspect. Perhaps they were discouraged by Dr. Hamer's unwillingness to accept the meningococcus as the cause

[1] *See* "The Treatment of Epidemic Cerebrospinal Meningitis," by G. C. Low, M.D., *Brit. Med. Journ.*, February 27, 1915.

[2] *See* Report of Medical Officer, 1913-14, Local Government Board, Appendix A; No. 2, "Summary of Cases of Poliomyelitis and Cerebrospinal Fever notified during 1913" (Reece, p. 2).

of cerebrospinal meningitis. The Section knew its Dr. Hamer and was used to his views, and they must not be dismayed on this account. Dr. Hamer had extolled the perspicacity of Sydenham, but he took it that the profession should not abandon all the knowledge which had been derived since that great man died. Dr. Hamer had often shown justifiable scepticism as to some of the conclusions drawn by enthusiastic bacteriologists. A healthy scepticism applied with Dr. Hamer's conspicuous critical acumen was no doubt good for them, but his persistent want of faith was pathological and denoted stiffneckedness. An epidemiologist who repudiated the findings of bacteriology was, he submitted, much in the position a physician would be who refused to avail himself of the knowledge which had been gained with regard to the circulation of the blood.

As emphasised by Dr. Arkwright, in considering an epidemic of cerebrospinal meningitis, it was necessary to include all the people who had the infective agent in their nasopharynx. It had been said that from ten to forty persons were so infected to each one who had meningitis. So long as the meningococcus was tucked away in the spinal canal it was not a source of danger to anybody other than the patient, and it was the carriers at large who were the principal source of infection. Could one tackle them? Theoretically it was clear that if one wished to check the epidemic these individuals must be hunted out and isolated as best they could be. But was that practicable? On that point some of the experienced sanitary authorities present might give some help.

Captain JOHN DALE, R.A.M.C., said that men in uniform had received advice, if not orders, as to the methods to be adopted, and had followed that advice; but he had heard the opinion expressed, and agreed with it, that the present policy of isolation of contacts was of very doubtful value. He asked members to consider what happened in small towns when it was adopted, troops being billeted, what alarm and despondency were caused by such a measure. He asked how often Dr. Arkwright swabbed the 800 men among whom he found thirty carriers. [Dr. ARKWRIGHT: Once only.] He thought it a pity the disease was called cerebrospinal meningitis. Of the twenty cases he had recently seen, four or five had been septicæmic, and in two the germ had been recovered soon after death from the heart blood, the meninges showing nothing but slight congestion. Cases of meningococcal septicæmia might occur without involvement of the meninges. Reference had been made to a non-epidemic time, but, as

Dr. Robertson said, the cases might be missed to a large extent in ordinary times. He could mention an instance of that. In the district from which he came, the medical officers from a town a considerable distance away came, by request, to see cases where he (the speaker), was stationed, so that they might distinguish the cases more readily. Next day a man, aged 45, died in their own hospital who had been ill a week, diagnosed as influenza and delirium tremens, and one of his (the speaker's) staff went over and did a post-mortem examination. He found meningococcal meningitis, and discovered another case among the patients in the hospital. There had been cases in hospital under special observation for a week before lumbar puncture appeared justifiable; and he felt sure that in ordinary times a large proportion of cases would be missed.

Dr. TUNNICLIFFE called attention to a notice which had been issued by the Medical Officer of Health of a provincial city to the effect that the public were urgently advised to submit to preventive vaccination for epidemic cerebrospinal meningitis, and expressed the opinion that, in view of the fact that preventive vaccination for this disease was not recommended either by the War Office or the Local Government Board in their respective recent memoranda, the notice in question was worded too strongly and was to this extent misleading.

Dr. CARNEGIE DICKSON said the last meeting left off when the topic introduced by the last speaker was under discussion, and he had come to hear other bacteriologists on the subject, because he had spent strenuous days during the last week or two trying to get meningococci from the throat. From one case, which died ten days ago, he had examined contacts very carefully, making over a hundred cultures, but without finding the meningococcus. But he grew the organism from the fatal case, and asked whether he was justified in vaccinating all these contacts with the dead patient's organism. In the Edinburgh Children's Hospital one got a case every few weeks, but he had only investigated the fatal cases; the others were sent to the Fever Hospital. A small proportion of the fatal cases showed the presence of another organism, so that in a few of the cases one might have to deal with a mixed infection. A so-called "leptothrix" had been mentioned as an associated organism, and he had found that in four cases diagnosed clinically as cerebrospinal meningitis. These complicating organisms might increase the severity of a case and cause lack of response to serum treatment. In other forms of meningitis mixed infections were

not uncommon, pneumococci, streptococci, &c., being not infrequently found in tuberculous meningitis. He had never found the meningococcus in tuberculous cases. He wondered whether Dr. Arkwright had had any of these accidental mixed cases. During the last few years he had had four cases sent him with the diagnosis of cerebrospinal meningitis, in which the organism was a pure "leptothrix." In two cases the organism was *Bacillus paratyphosus* B. From the morbid anatomy of the brain alone one could not distinguish some of these other forms of purulent meningitis from the meningococcal, therefore bacteriological examination of the cerebrospinal fluid was absolutely essential. He asked whether, on the basis of the knowledge of the Glasgow epidemic, cases had been treated in the present epidemic intrathecally with their own serum.

Dr. D'Este Emery said that the differentiation of the meningococcus from the gonococcus was a matter which sometimes caused the bacteriologist some embarrassment, for gonococcic meningitis occurred, though rarely, and the diagnosis of the exact nature of meningitis occurring in a patient suffering from a gonorrhœal infection (as in a patient of his, who had recently had a gonorrhœal abscess of the prostate) was by no means easy. Dr. Emery thought that the case mortality of cerebrospinal meningitis had been put much too high, owing to the non-recognition of mild cases; the disease was devoid of characteristic symptoms, and if modern methods of diagnosis were not resorted to, only the severe cases were recognised. Everyone who had done large numbers of lumbar punctures at a children's hospital would agree that the employment of this measure as a routine procedure led to the recognition of many cases of meningitis which ran a short and favourable course, and which would in the ordinary way be regarded as influenza, rheumatism, bronchitis, &c. He thought the mortality suggested by some authorities, 70 per cent., much too high; probably a half of this would be an outside figure. In opposition to a previous speaker, he stated most emphatically that secondary or mixed infections were rare in the extreme in any form of meningitis. He had only seen one case, the meningococcus occurring as a secondary infection in the course of tuberculous meningitis. As regards the incubation period, he had seen a case of basic meningitis in which the symptoms came on two or three days after birth.

Dr. G. S. Buchanan, referring to the President's question of the practicability of isolating the meningococcus carrier, said that it

appeared to depend very much on the magnitude which the problem had assumed in the particular case; for example, the phase of the epidemic and the extent to which meningococcus infection (as distinct from meningitis) was prevalent in the community to be dealt with. Dr. Arkwright's results seemed to show that, in a particular military unit, over 3 per cent. of carriers could be found at the end of some months of prevalence. Now, if it was accepted that in the ordinary temporary carrier the meningococcus was only present for two or three weeks, it must be concluded that a very large proportion of all the men in that unit had been carriers at one time or another during the months in question. If similar conditions arose and had to be dealt with in a civil population, strict isolation of every carrier from all contact with his fellows would be impracticable; though precautions could still be enjoined in respect of known or suspected carriers in such matters as the use of handkerchiefs or drinking vessels, avoiding kissing, and sleeping apart. On the other hand, it might be practicable to adopt stricter measures with known or suspected carriers on the occurrence of a first case of cerebrospinal meningitis in a locality previously free, and particularly with those who were actually suffering from catarrh.

Dr. Hamer's observations on the coincidence of increased meningitis prevalence with increased mortality from influenza, bronchitis, and pneumonia, and his suggestion that in a broad sense all these were part of one general epidemic were valuable; it was quite possible that symbiosis was underlying the increase of meningococcus infection. He hoped that more observations would be communicated on the development of meningitis in the known meningococcus carrier. Wider knowledge on this matter would have an important bearing, not only on practical administration, but also on the problem of individual susceptibility to meningitis.

Dr. A. Mearns Fraser (Portsmouth) said that, as a medical officer of health, he had been anxious to hear more as to the cause of the disease. In Portsmouth, in the naval, military and civilian population, there had been altogether forty cases, of which about half had been amongst civilians. The first case notified was a soldier, on January 28; this had been followed by cases amongst sailors at the Royal Naval Barracks, and amongst recruits at the Royal Marine Artillery Barracks. The first case amongst civilians occurred on February 11 in a child, aged 11, notified after death. Inquiries elicited the fact that this child attended the Eastney Barracks School, which was in regular use each day, after the children had left, by recruits of the Royal Marine Artillery,

amongst whom several cases of cerebrospinal meningitis had occurred. Subsequently four other children who attended this school were attacked. Secondary cases in the same house had occurred in three instances—two of these were children in private houses, and the third occurred in an empty house that had been taken for billeting, and in this house the second case was in the same room occupied by the first case. One case, a sailor, at the Naval Barracks, appeared to have contracted the disease at the Crystal Palace, as the patient had arrived from there four days previously. He (Dr. Fraser) had found it difficult to establish a connexion between the cases except in eight instances, but he had been particularly struck by the fact that in a large proportion of the civilian cases there had been an outbreak in the house of influenza with severe headache a week or two previous to the case of cerebrospinal fever. He was somewhat sceptical as to the value of throat swabbing and bacteriological examination of contacts; it seemed that the number of contacts who had meningococci in the posterior nasopharynx might vary from 40 per cent. or even 60 per cent. to 1 per cent., according to the observer who conducted the examination; if he had to submit swabs from contacts for examination he must confess to a preference for the bacteriologist who only found 1 per cent., because in civilian practice, if contacts were discovered, medical officers of health would not know what to do with them. For certain it was that people would not agree to be isolated because you assured them they had a dangerous organism called the *Diplococcus intracellularis* in their nose. He agreed to a large extent with the views expressed by Dr. Robertson, and he thought there were a number of deaths each year certified to be due to meningitis or from influenza and convulsions, of which the symptoms were indistinguishable from those of cerebrospinal fever. The disease was a seasonal one and it occurred to him that possibly it only became prevalent when its seasons —the winter and spring—were marked by certain exceptional meteorological conditions, such as excessively cold winds or heavy rainfall, and combined with these possibly individual conditions, such as fatigue, or overcrowding, or bad ventilation. In this connexion he thought it might prove valuable to have a record of the meteorological conditions during the Glasgow, Belfast, and other outbreaks.

Section of Epidemiology and State Medicine.

April 23, 1915.

Dr. C. J. Martin, F.R.S., President of the Section, in the Chair.

Small-pox among Cotton Operatives.

By H. E. Corbin, B.Sc., D.P.H.

During 1908 and 1909 cases of small-pox occurred in the borough of Stockport in which, after careful inquiry, there was evidence of infection being conveyed through the medium of raw cotton from foreign countries where small-pox is endemic.

The first case occurred in a woman, aged 32, unvaccinated, who was employed in the card-room of a cotton mill. Careful investigation into the probable origin of this case, her very limited sphere of life, and the fact that no case of small-pox had recently occurred in the neighbourhood or indeed in the country, led me to suspect the possibility of the infection arising out of the nature of her employment. Her occupation was that of a combing tenter, and one of the important duties of these operatives is to perform the operation of "piecing." The strands of raw cotton in passing over the frames frequently break and the process of piecing consists of hauling in a certain amount of slack in order to overlap the loose ends and then rolling the overlapping ends together between the fingers and thumb or between the palms of the hands. It is a common practice among these operatives to moisten the hands with saliva in order to facilitate the process of piecing, and I have frequently observed card-room workers introduce the thumb and forefinger into the mouth when joining the finer strands together, or lick the palms of the hands when piecing the coarser strands of the raw cotton. I ascertained that this woman also was in the habit of using saliva for the purpose of piecing.

Should any infection exist in the raw material thus dealt with it is easy to see how it is possible for infection to be conveyed directly to the mouth of the operative handling it, and how infected material which might be quite harmless with ordinary handling might become dangerous when the infection is thus introduced into the system of a susceptible individual. Having regard to the incubation period and the time of arrival of the raw material in England, I ascertained that the consignment of cotton which might have given rise to this case of small-pox came from Memphis, and was shipped from New Orleans; moreover, while small-pox is endemic in these parts, it was more than usually prevalent at the time when the supposed infective consignment was gathered and imported.

This case was discrete and mild, and it is interesting that no secondary cases arose from it.

In February, 1909, three other cases of small-pox occurred at Stockport. The primary case was a woman, aged 21, employed in the card-room of a cotton mill. She had been vaccinated in infancy. The doctor who attended the case was doubtful as to the diagnosis, and after consultation with his partner concluded that it was chicken-pox, so that the case was not discovered until the first contact case was notified. This was a severe semi-confluent case occurring in a friend who had visited the first case when she was ill. The second contact case was the mother of the primary case. The occupation of the first case was that of a drawing tenter, and this operative also was in the habit of moistening the hands with the tongue in order to facilitate the joining of the broken ends of the raw cotton as it passed over the drawing frames.

I ascertained from the manager of the mill that the cotton in use at the mill was Egyptian, and that it was grown at Zifta, in the delta of the Nile. The cotton which possibly contained the infection was shipped from Alexandria and received at the mill about the end of January. I further ascertained that small-pox had for some time been very prevalent in the delta, a large number of deaths occurring both at Cairo and Alexandria. Dr. Ross, Medical Officer of Health for Cairo, was kind enough to send me a statement showing the incidence of small-pox in Cairo, Alexandria, and Port Said during the four years 1906-09. He states that "the period of comparative immunity from small-pox in Egypt is now about nine years. The last outbreaks began in Alexandria early in 1907, when 504 cases occurred. It then spread to Port Said in March, 1908, and to Cairo in December, 1908, where

during the first half of the year 1909, 447 cases were notified, with 200 deaths."

Dr. Ross adds: It should be noted that very little cotton is exported from Cairo itself, for the majority is packed in the country districts and exported mainly from Alexandria."

It will be seen later that the greatest danger of the raw cotton becoming infected with small-pox exists in the country districts and probably very little infection occurs at the exporting towns.

I drew attention in the Stockport Health Report, 1908, and in *Public Health*, 1909,[1] to the possibility of the infection of small-pox being conveyed from foreign parts by means of raw cotton, and several minor epidemics have since been reported in which there was evidence

TABLE I.—SMALL-POX IN CAIRO, ALEXANDRIA, AND PORT SAID.

Date	Cairo		Alexandria		Port Said	
	Cases	Deaths	Cases	Deaths	Cases	Deaths
1906	36	9	80	41	7	1
1907	62	24	504	283	18	7
1908	269	95	35	19	42	19
1909 (January to July)	447	200	—	—	—	—

of a similar source of infection, and I have obtained interesting information concerning some of these outbreaks which bears upon the subject.

At Heywood, Lancashire, in January, 1910, seven cases of small-pox occurred among the workpeople at a cotton mill; and I am indebted to Dr. Hitchon, Medical Officer of Health, for information concerning these cases.

It is a significant fact in this small outbreak that two men employed at the mill contracted small-pox simultaneously, the rash appearing in each case on the same day. These two men, who were the primary cases in the outbreak, were employed at the same machine, each being a stripper and grinder. The raw cotton from the blowing-room is placed in the carding engine and passed over revolving cards, from

[1] "Small-pox from Imported Cotton," *Public Health*, 1909-10, xxiii, p. 20.

which innumerable small wire spikes project: and by this means it is disentangled, combed and cleaned, the good cotton fibre adhering to the cards, the waste, dust and dirt passing into boxes below. The cotton is removed from the cards at intervals by the strippers and grinders, and the cards require frequent cleaning on account of the dust and dirt

TABLE II.

No.	Age	Sex	Occupation	First symptoms	Left work on	Specific symptoms	Number of spots	Spots appeared on
				Primary Cases.				
1	42	M.	Stripper and grinder	Cold: December 28, 1909	December 31, 1909	Headache, backache, vomited; scarlet fever rash on chest and abdomen, January 3, 1910	Semi-confluent	January 5, 1910
2	43	M.	Do.	Run down; December 31	January 4, 1910	Headache, backache, sickness	About 40	January 5, 1910
				Contact Cases.				
3	34	M.	Odd man	Run down: January 9	January 14, 1910	"Pains all over," headache, vomiting	About 100	January 18, 1910
4	20	F.	Rover	Headache: January 12	January 14, 1910	Stiffness of joints, headache, vomiting, and shivering	About 70	January 16, 1910
5	37	F.	Slubber and tenter	Headache and vomiting: January 13	January 14, 1910	Severe headache, backache, and vomiting	About 100	January 18, 1910
6	45	F.	Drawing tenter	Felt sick: December 23	January 14, 1910	"Pains all over," headache, cough	About 5	January 19, 1910
7	17	M.	Card-box tenter	Pains in abdomen: January 12	January 17, 1910	Very severe pains in abdomen, headache, and vomiting	Confluent	January 19, 1910

which collect between the small projecting spikes. Although it is compulsory now to clean the carding engine by machinery which carries away the bulk of the dust, in the cases under consideration the cleaning of the cards was carried out by the strippers and grinders by means of hand brushes, and from the large volumes of dust thus occasioned it would be impossible to avoid inhaling a considerable quantity. As the result of careful inquiries no evidence whatever could be obtained that either the employés themselves or the cotton could have become infected

by any unsuspected case of the disease working at the mill. A common cause of infection outside the mill was also excluded, as the two men lived at a considerable distance apart, and never met except during their work at the mill.

The cotton used at the mill was Egyptian, but it was not possible to ascertain the exact source from whence it came. The primary and contact cases had, with one exception, all been vaccinated in infancy.

The fact that two employés working at the same machine contracted the disease at the same time, when every source of infection outside the mill can be excluded, indicates strongly that the material upon which they were working was the common source of infection.

At Colne, in July, 1910, an outbreak of small-pox occurred. The Medical Officer of Health informed me that all the cases were workers in two spinning mills, but he was of opinion that the source of the disease was not associated with their occupation, and supported his view by the argument frequently brought forward, that having regard to the amount of cotton dealt with in Lancashire, if the raw material were infected there would be greater epidemics of small-pox. The primary case in this outbreak had not been reported and was discovered to have fresh scars, when other contact cases had been notified.

The cotton in use at the mills was American, some coming from Memphis, U.S.A.

I was unable to obtain detailed information as to the exact occupation and circumstances of these cases, and although there is little evidence to inculpate cotton as the causal factor, I have mentioned the outbreak as it originated among and was limited to cotton operatives.

At Chadderton, Lancashire, a small outbreak affecting six persons commenced in November, 1910. The first case, a female aged 14, unvaccinated, was employed as a little tenter in the card-room of the Manor Mill, Chadderton. Dr. Ashton, who kindly supplied me with the information concerning these cases, stated that, "in her work she might moisten her fingers to piece the ends, and in any case there is plenty of dust and cotton fibre in the air which would find its way into her mouth; I was unable to find any other source of infection unless some infected article from our last epidemic which, however, finished four years ago."

All the cotton used at the Manor Mill came from Memphis, U.S.A.

The remaining five cases, of which three were cotton operatives and two were children, were all connected in some way with the first case,

and would appear from the dates of onset of illness to be contact cases. The medical officer remarks that "Case 1 is the only one I can attribute to direct infection from cotton."

In March, 1911, an epidemic of small-pox commenced at Bury, resulting in the notification of thirty cases. Information concerning this outbreak is given in the Annual Health Report for Bury by Dr. Granville Buckley, and it may be interesting to quote the following extracts: "In the first instance only two cases were notified—the mother (Case 1) and daughter (Case 2)—and, on inquiry at the home, it was found that another daughter (Case 3) was being treated by her medical attendant for a skin disease. This girl, aged 15, was unvaccinated, and was employed as a weaver in a cotton mill, where she had been engaged nine days. Prior to commencing work as a weaver she was engaged in the card-room of another local mill, and it was at this mill that she commenced to be ill. On examination, she was found to be suffering from small-pox, and in about the third week of the disease. In the same weaving shed in which the girl had worked were 300 operatives, who must have been in contact with this unrecognised case of small-pox."

"The probable source of the outbreak was infection from cotton, but at the same time no direct evidence could be traced to this."

It was, of course, urged in objection to the view that the infection was conveyed in raw cotton, that the operative first attacked was a weaver, and that the probabilities were greatly against cotton retaining any infection up to the weaving stage; but when the patient began work at the weaving shed, where she was found, she already had the disease, and was previously employed in another mill as a can-tenter, and in that capacity came in contact with the raw cotton in an atmosphere impregnated with cotton dust, and, assuming there was infection in the cotton, there was every liability of a susceptible person inhaling it. All the cotton the girl had to deal with came from the Southern States. It may be stated here that the man who dealt directly with the cotton from the bales had only been vaccinated once, but he showed extremely good cicatrices on his arm. The infection (still assuming that it was in the cotton) was probably too attenuated to affect a vaccinated person; and it was unfortunate that a totally unvaccinated girl should have been in contact with the raw material. At the time the girl changed her employment, and began weaving, there were pustules all over her face. These were seen by her fellow-operatives."

At Oldham, in 1913, a small outbreak of small-pox occurred, in which the first patient to be attacked was a girl who was employed in the card-room of a cotton spinning mill. The raw cotton used at this mill came from the Southern States, and the medical officer of health was of opinion that cotton was the source of infection.

At Milnrow, Lancashire, in May, 1914, another outbreak of small-pox of a highly interesting character occurred, associated with infection in a cotton mill; and Dr. Macewen, of the Local Government Board, who investigated this outbreak, has kindly consented to add some notes concerning these cases as a supplement to this paper.

The important question now arises as to how and to what extent the raw cotton may be specifically infected.

In the report of the New Orleans Board of Health for 1907, referring to small-pox, it is stated that "the bulk of the infection was due to repeated importation of cases both in the incubative and eruptive stage, the spread of infection from individual cases being limited in most instances to one or two cases of the disease at the most." While prompt measures are taken to deal, both with cases which arise within the city and those which enter from outside—and, indeed, with very good effect, as usually only one, or at most two, contact cases are reported to arise from these cases—it is evident from the above statement that a large number of persons actually suffering from the disease in the eruptive stage, as well as others in the incubative stage, enter the towns from the outlying districts. The mild character of the disease in New Orleans in 1907 is indicated by the fact that among 234 cases of small-pox reported only ten deaths occurred.

A prevalent superstition is current among the field hands in the States that the white "medicine man" is possessed with the occult power of witchcraft, with the result that many who are smitten with disease of any kind prefer to hide away and take their chances unattended rather than come within the grasp of the doctor. Many cases of small-pox occurring among workers in the cotton fields are thus unheard of.

Although precautions are taken as far as possible to prevent infected persons from working in the cotton fields, owing to the expanse and undeveloped state of the great part of the Cotton Belt the difficulties of adequate Public Health administration at the present time are very great, and when small-pox, especially of a mild character, is prevalent among the hands there is no doubt that many cases deliberately unreported as well as "missed" cases continue to work in the cotton

fields throughout an attack of the disease. In the process of gathering the raw material in the cotton fields the pickers, who are negroes of both sexes, carry large open-mouthed bags, which are slung around their necks, and hang in front of the body. They gather the cotton with both hands as they pass down the rows in the cotton field and deposit it in these bags. An almost universal habit obtains among the pickers of both sexes of chewing tobacco, and in the States a root known as waheyawa, while at work; this engenders the habit of frequent expectoration, and commonly some of the expectorated matter enters the bags containing the cotton, so that should a picker while suffering from small-pox continue to work in the cotton fields it is clear that, apart from merely handling the cotton, these undesirable habits of chewing and expectoration constitute a direct means of specifically infecting the raw material as it is being gathered in the fields. Infection may also occur, though in a less degree, in the process of ginning and in baling or packing the cotton for transport at the docks.

With regard to the transmission of the infection of small-pox, it is acknowledged that under certain circumstances the "striking distance" and infectivity of this disease may be very great, probably greater than that of any other infectious disease, and that the contagium may be transmitted by aerial convection over a considerable distance, and while generally speaking the disease is communicated by more or less intimate contact with an infected person, it is admitted that fomites and other infected materials are able to transmit the disease. Infected material, however, appears as a rule to become ineffective in a comparatively short time as a means of spreading the disease, unless the conditions are such as to constitute a suitable nidus for the contagium. It is pertinent therefore to consider these conditions in the case of raw cotton.

Bales of cotton which are damp frequently command a higher price on account of their appearance than those, containing the same quality of cotton, which are dry, and some experienced merchants, while deprecating excessive damp, hold the opinion that cotton that does not contain moisture cannot be properly baled; hence it is a common practice to adulterate the cotton before it is pressed into bales by the addition of water, and at a Congress of Cotton Spinners in Egypt, in October, 1913, Egyptian exporters were severely criticised by the delegates who had inspected the baling of the cotton at the quayside in Alexandria, and had noticed that the contents of a large syringe holding about a quart of water were from time to time sprayed on the loose cotton. The

normal amount of free moisture in raw cotton has been ascertained to vary from about 8 per cent. to 12 per cent.

When the bales are opened at the spinning mill the interior is often found to be quite warm, and no doubt slight fermentative processes are going on which maintain a certain degree of warmth in the interior of the bale from the time it leaves the pressing house at the foreign port until it reaches the mill in Lancashire. Raw cotton therefore, on account of its texture and organic nature as well as the conditions of moisture and temperature which have been shown to exist within the bales, is probably a very suitable medium for the saprophytic existence of the contagium of small-pox, and for the transference of the disease from foreign parts.

The question is frequently asked why, if small-pox can arise from imported cotton, considerable epidemics do not frequently occur in Lancashire?

The reply is that outbreaks of small-pox do, in fact, frequently occur in Lancashire, much more frequently, indeed, than in any other part of the country, and with the exception of occasional cases arising from Liverpool shipping, nearly always originate among cotton operatives. The question why these outbreaks do not develop into considerable epidemics is one which may be applied to any other infectious disease, although there are certain features which characterise these outbreaks relating to the vitality of the germ, the quantity of infecting material, and the susceptibility, or its converse, the resistance of the worker, which should be specially considered. It is an interesting fact that the primary cases which appear to have arisen by direct infection from the cotton are as a rule of a mild type, whether vaccinated or not, and on more than one occasion have been mistaken for skin disease or chicken-pox until contact cases have revealed the presence of small-pox. Moreover, the infectivity of the primary cases does not generally appear to be great, having regard to the large number of cotton operatives who come into more or less intimate contact with these cases, and the usually limited character of the outbreaks, the number of cases occurring in each of the outbreaks I have mentioned being as follows:—

				Primary	Secondary
Stockport	...	March, 1908	...	1	0
Stockport	...	February, 1909	...	1	2
Heywood	...	January, 1910	...	2	5
Colne	...	July, 1910	...	1	(?)
Chadderton	...	November, 1910	...	1	5
Bury	...	March, 1911	...	1	29
Oldham	...	— 1913	...	1	7
Milnrow	...	May, 1914	...	11	0

Leaving out the question of preventive measures, which are of course energetically pursued in every case, it will be seen that the first Stockport case was an isolated one, and that eleven cases at Milnrow, which appear to have been independently infected by cotton, did not give rise to any contact cases. In the outbreak at Bury the primary case, owing to an error in diagnosis, had been working for nine days in a weaving shed with 300 operatives before the disease was recognised.

While suitable conditions for bacterial life have been shown to exist in baled cotton it is probable that the organism of this disease becomes attenuated in this extra-corporeal habitat, and that the infectivity of the material may be considerably reduced during the prolonged interval between the gathering of the cotton and the manufacturing processes, and that this reduction in vitality as regards virulence, reproductive capacity and striking distance of infection may to some extent account for the limited character of the outbreaks.

With regard to the quantity of infecting material, generally speaking, infection by the native would only occasionally occur, and hence a very small proportion of the enormous amount of cotton exported can thus be affected; moreover, only those operatives who from the nature of their occupation inhale a considerable quantity of dust, or who might introduce infection into the mouth in the process of piecing, appear to be liable to contract the disease from the cotton.

The early processes of manufacture consist entirely of methods for cleaning out and loosening the matted masses of fibres, and enormous quantities of dust, consisting of sand, leaf, seed, fibre and other impurities are removed. These processes are carried out in the mixing room, the blowing room and the carding room, and although the bulk of the dirt is removed by the machines the atmosphere is often impregnated with dust, especially when the machines are cleaned out by hand. In the card-room also are various machines, the drawing frames, combing frames, slubbing frames and roving frames, by means of which the cotton fibre is further cleaned, rendered parallel and drawn to a fine thread. At these machines piecing is done by the various tenters or attendants at the frames. The operatives engaged in these rooms appear to be especially liable to infection, while the later processes of manufacture in the spinning room are apparently devoid of danger.

I have never heard of a case of small-pox originating in a weaving shed. The cotton used for this process is no longer in the raw state and no danger of infection arises in this occupation.

The resistance of the worker is of course an important factor which

operates in determining the frequency and magnitude of these outbreaks. Before the Vaccination Act of 1907 the percentage of exemptions in England and Wales was 8·4, and though a higher percentage would obtain in Lancashire, it may be safely assumed that not less than three-fourths of the operatives working in the mills at the present time were vaccinated in infancy. The residual protection existing as the result of early vaccination undoubtedly has an effect in limiting the outbreaks. At the present time in many of the manufacturing centres exemptions from vaccination are being obtained for more than half of the total births, so that the number of persons liable to infection from small-pox is rapidly increasing, and hence outbreaks of the disease among cotton operatives are likely to become more frequent, although improvements in the working conditions and the manufacturing processes may to some extent counteract this tendency. But in view of the risk of infection incurred by operatives in dealing with raw cotton I think it is justifiable that the vaccination or re-vaccination of these employés should be made compulsory.

Summary.

(1) Small-pox has been shown to be prevalent in certain cotton-growing districts.

(2) Missed and unreported cases occur among workers employed in the cotton fields.

(3) Occasional infection of the cotton by the worker is inevitable.

(4) The raw material from these districts is imported for manufacture to mills in Lancashire.

(5) Certain employés working with this cotton contract small-pox when other sources of infection can be excluded. In one of the outbreaks mentioned, two cases dealing with the same raw material, and in another eleven cases, were infected simultaneously.

(6) These facts point directly to the conclusion that small-pox is conveyed from the worker in the cotton-growing districts to the cotton operative in this country, and that raw cotton is the vehicle of infection.

(7) Only those employés engaged in the earlier processes of cleaning and combing the raw material appear to be liable to direct infection from this source, and no risk attaches to the occupation of weaving.

(8) Outbreaks of the disease arising from cotton appear to be mild in character and limited in extent. This may to some extent be accounted for by the fact that the majority of employés have been

vaccinated, albeit in infancy. The mild character, however, is also revealed when the primary case is unvaccinated, so that it is not unlikely that some loss of vitality of the infecting organism may take place during a saprophytic existence.

(9) The evident danger of infection from raw cotton would be entirely obviated by compulsory vaccination.

Brief Note on Cotton as a Source of Small-pox Infection, as illustrated by an Outbreak of the Disease in Milnrow Urban District and the adjoining Borough of Rochdale.

By Hugh A. Macewen, M.B.

In June, 1914, I visited the Urban District of Milnrow, in Lancashire, and the Borough of Rochdale, for the purpose of making inquiry concerning the circumstances of a small outbreak of small-pox. It may be convenient to deal with the Milnrow cases first, as the disease first appeared in that district. There were eleven cases of small-pox in Milnrow, and the dates on which the rash first appeared were as follows: May 30, four cases; May 31, five cases; June 1, one case; June 2, one case (eleven cases in four days). It appeared, therefore, that there was some common source of infection. All of the persons who contracted the disease were employed in the New Ladyhouse Cotton Spinning Mill. So far as could be ascertained, none of them had been recently out of the district, and no source of infection outside of the mill could be suggested.

The New Ladyhouse Mill is divided into two separate parts. In one part the bales of cotton are opened out, and a series of processes are gone through ending with the spinning of the cotton; whilst in the other the cotton is finished in a variety of ways. All the workers who contracted the disease worked in the first of the two parts, where some 132 persons were employed, while only some eighteen persons worked in the second part in which no cases occurred. The cotton passes through the following processes in the first part of the mill in which the infected workers were employed:—

(1) *Mixing Room.*—The raw cotton is elevated to the mixing room, which is on the first floor directly above the blowing room. Two men were employed in the mixing room in opening the bales and

blending different kinds of cotton ("American," Brazilian, Peruvian, Mexican, &c.) and in passing the blended cotton into the bale-breaking machines. One of these two men contracted small-pox.

(2) *Blowing Room.*—From the bale-breaking machines the cotton passes into the blowing room beneath, where two men work. More dust is probably produced in the blowing room than in any other part of the mill, and the blowers are brought more intimately in contact with it, seeing that they have to clean out the dust by hand from the machines three times a day. This is significant, as both "blowers" took small-pox.

All the cotton in the mill passes through the hands of the four men employed in the preliminary processes of mixing and blowing, and three out of the four contracted the disease.

(3) *Carding.*—The cotton from the blowing room is carried into the main building by four card-room jobbers, who also attend to the carding engines. One of the card-room jobbers became affected with small-pox.

(4) *Can-breakers.*—Two can-breakers were employed in conveying cans of cotton from the carding engines to the drawing frames. One became affected with small-pox.

(5) *Drawing Frames.* — Four women and four assistants were engaged in attending to the drawing frames. One of them contracted small-pox.

(6) *Slubbing Frames.*—The cotton next passes through the slubbing frames at which four slubbers and four assistant slubbers work. Three of the slubbers contracted the disease.

(7) *Intermediate Roving Frames.*—Six women are employed at intermediate roving frames, none of whom contracted the disease.

(8) *Roving Frames.*—Eight women work at the roving frames, one of whom contracted small-pox.

(9) *Ring Room Operatives.* — One out of some ninety ring room operatives became infected with small-pox.

It will be observed, therefore, that the incidence of infection was heaviest among the workers employed in the earlier processes. Nine processes have been described, and of the ten persons employed in the first four, five became affected with small-pox, or 50 per cent.; whereas, of the 120 persons employed in the five later stages of manufacture only six persons, or 5 per cent., contracted the disease. It is interesting to note that three out of eight persons employed at the slubbing frames—which is one of the later processes—should have become affected with small-pox, and I am unable to give any reason as to why such a relatively large number of slubbers should have been attacked.

There is no reason to suppose that these workers not attacked by small-pox were, on account of immunity acquired as a result of vaccination or through a previous attack of small-pox, any less susceptible to the disease than those who suffered attack. All the patients, with one exception who had never been vaccinated at all, had been vaccinated in infancy, but not re-vaccinated. I examined sixty-two of the workers who had not become affected with small-pox; forty-four had been vaccinated in infancy, five had been vaccinated in infancy and re-vaccinated on one or more occasions, and thirteen were entirely unprotected by vaccination at the time when small-pox first appeared.

While it is difficult to explain the source of infection on any other hypothesis than that it was conveyed by cotton from the country where that product is produced, two other suggestions seem possible—i.e., the presence in the mill of an unsuspected case of small-pox, or the handling of cotton in the early stages of manufacture by someone in an infectious condition.

Careful inquiry as to the habits of the mill operatives showed that they did not come closely in contact with one another, either at meal-times or in any other way. It would seem, therefore, that if small-pox was conveyed by personal infection it must have been through the agency of someone whose duty brought him in contact not merely with all the operatives in the main building but also with those in the detached mixing and blowing rooms in which three cases of the disease occurred. The only three persons who could have fulfilled these conditions were the manager, the card-room overlooker, and the assistant card-room overlooker, and inquiry showed that no exception could be taken to their state of health during the previous few months.

The second suggestion, that cotton handled in the early stages of manufacture by someone in an infectious condition might be responsible for the outbreak, can be readily dismissed, as only four men working in the mixing and blowing rooms are implicated on such a hypothesis. Three of these need not be considered, as they contracted small-pox with the other operatives, leaving only an old man who, though not in the best of health, had had no symptoms which were in any way suggestive of his having had small-pox. There would, therefore, seem to be evidence that the virus of the disease was introduced by cotton from the country where it was produced.

As already stated, the cotton used at the mill was derived from a number of different sources. I was informed that Mexican cotton was used for the first time on May 14. Allowing an incubation period

of fourteen days, May 28 is arrived at, and the rash was first observed in the majority of cases on May 30 and May 31, whilst one case sickened on May 26, five on May 27, one on May 28, three on May 29, and one on June 1. This is a suspicious circumstance, though the evidence is insufficient definitely to inculpate the Mexican cotton and to free that from other sources from suspicion.

Rochdale Case.

On June 3 a case of small-pox was notified in Rochdale. At first no connexion could be traced between the Milnrow cases and the Rochdale case. It was subsequently found, however, that the man worked in a "waste" cotton mill in Rochdale, and that this mill had a contract for the supply of waste cotton from the New Ladyhouse Mill at Milnrow. The last consignment of cotton waste was sent from Milnrow to Rochdale on May 18. This included all the waste cotton which had accumulated as the result of working at the New Ladyhouse Mill for a fortnight before May 18, it being customary to send the waste regularly once a fortnight.

The Milnrow operatives must have been infected by cotton passing through the mill on or about May 13 or May 14, and, as already stated, the first Mexican cotton was used at the Milnrow mill on May 14. The waste from this infected cotton was sent to Rochdale on May 18, and would probably be made use of on that day or a few days later. The rash appeared on the Rochdale case on the evening of Wednesday, June 3—sixteen days after the arrival of the waste cotton from Milnrow. It is significant, also, that the man attacked was a "mixer," who was the first to handle the waste at the mill. There seems, therefore, to be strong evidence of the cotton waste having conveyed infection to Rochdale, and it is difficult to explain the occurrence of the case on any other hypothesis. It should perhaps be added that, in Milnrow, all the small-pox cases were employees at the cotton mill, and there were no secondary cases. Some of the Milnrow cases, as well as the Rochdale case, were isolated in a ward in the infectious disease hospital, Rochdale, and there was spread of the disease to inmates of houses in the vicinity of the hospital.

DISCUSSION.

Dr. J. B. WILKINSON: I have always until recently been somewhat of a sceptic that cotton is the cause of this disease. Oldham is a town in which the staple trade is mainly cotton spinning, and the cotton, of course, comes into the town in a raw state. This cotton comes from districts where it is well known that small-pox exists, and where also workers in the cotton fields have been seen working while suffering from the disease. We have here all the possibilities for the conveyance of the disease, and yet until the years 1913, 1914 and 1915 no outbreak, with one or two possible exceptions, has occurred for which a definite origin has not been ascertained. From August, 1906, to the close of 1910, although cotton was coming in just the same, not a single case of small-pox occurred in the district. During the time I have been Medical Officer of Health until the outbreak of 1913 a definite origin has been traced for each outbreak, with the exception perhaps of the third introduction in 1904 and the second case in 1909. Why all these years should elapse without frequent outbreaks of the disease, when all the possibilities are considered, I am unable to give any opinion. During the last three years, however, *all* the cases which have occurred would seem to have cotton as the source of infection. The first case in all these outbreaks has been a worker in a cotton mill, and all of them persons who handle the cotton in the early stages of its manufacture. All the cases which have a possible origin in cotton have two characteristics—namely, that they are very mild cases, both in the original case and in the secondary cases resulting; the cases do not seem to be extremely infectious, and a very mild degree of vaccination usually protects the person. In this town a very much larger number of the adult population are now unvaccinated, and it is possible that as we have now in the mills a population who are not protected in any way, whereas in former times the majority of the workers had been vaccinated at some period of their life, this may be some reason for the more frequent occurrences of cases connected with cotton. All the primary cases in these outbreaks have, I believe, been unvaccinated persons. Appended is a brief summary of the cases of small-pox that have occurred in Oldham since 1898, with the origin:—

1898.—One case. A contact from a case in an outside district.

1899.—Two cases. The first case was the wife of a man who worked in a mill in an outside district. The man had some eruption about a fortnight previously. There was an indefinite history of contact of this man with another person who had recently come from Russia suffering from supposed chicken-pox.

1900.—Eight cases. The first person travelled from London to Manchester with a man returning from Russia with hæmorrhagic small-pox, who died a few days after his arrival at Stalybridge. The other cases were contacts of this case.

1901.—Two cases. The first case contracted the disease in Spain. The second case was a contact.

1902.—One hundred and seventy-eight cases. One case in April traced to contact with a case in another town. One case in April was a man who had recently come to a lodging-house from a similar institution in another town where there had been cases. Subsequent cases were directly or indirectly connected with this man.

1903.—Two hundred and fifty-six cases. Continuation of the epidemic from the previous year, with a further introduction by another tramp who came directly from a town where small-pox was prevalent.

1904.—Two hundred and fifty-five cases. Introduced in January from a neighbouring town and five cases. Six cases in March started by a tramp who had entered the town. A few cases in May started by a young woman who worked in a cotton mill in an adjoining district. In June, introduced again by a man working in Yorkshire. Introduced in August by a navvy from Yorkshire. This case was unrecognised and started a large epidemic which continued into 1906.

1905.—Two hundred and eighty-one cases. Almost entirely a continuation of the above epidemic, with three or four cases introduced from other sources.

1906.—Four hundred and fifty-five cases. A continuation of the same epidemic. In this epidemic there were somewhere about eight hundred cases, which were all of an exceedingly mild type and without a single death among the whole number.

1907-1908-1909.—No cases.

1910.—Two cases. Immediate contacts of a case in an outside district.

1911.—One case. A contact of the two cases of 1910.

1912.—No cases.

1913.—Eight cases. The first case was in March and was a girl who worked in the card-room of a cotton mill. There was one secondary case. Six cases in August. The first two cases went to work daily in an outside district where there were cases of small-pox at the time.

1914.—Four cases. The first case was a woman working in a cotton mill outside the town. Two other cases were contacts, and the fourth case was a man working in a cotton mill in the town.

1915.—The first case notified was on March 5, and all the subsequent cases, with one exception, were more or less directly connected with this man, who was an iron-worker, and had been ill a fortnight before the case was notified, on two days of which he had been to his work. On further inquiries it was found that about a fortnight previous to the commencement of his illness, the woman who took the washing from his house had an eruption which was diagnosed as blood poisoning. A fortnight previous to this woman's illness her sister had an eruption which was diagnosed as chicken-pox. This girl works in a cotton mill, and had some marks on her arms which were highly suggestive of a recent attack of small-pox. There were no other cases in connexion with this mill. The one case not connected with this outbreak was in another part of the town, where there was very little probability of connexion with the above case. He also was a worker in a cotton mill. It is a singular fact that although the mill was in another district from that one where the girl worked, the two mills belong to the same family, and on inquiry at the mill, though there was no connexion between the mills, the cotton bales were from the same consignment and purchased at the same time, some being delivered to the one mill and some to the other mill. The cotton was obtained from Norfolk, U.S.A.

Dr. G. S. BUCHANAN: These outbreaks originating in cotton mills bear a close resemblance to those which used to occur among rag-sorters and at paper mills where old rags were used. The Annual Reports of the Medical Officer

of the Local Government Board for 1881 and 1886 contain some instructive accounts by the late Dr. Franklin Parsons of such outbreaks. There also the earlier process—namely, the sorting of rags—was principally concerned with infection. Exceptionally the implicated rags came from distant countries—e.g., from Russia in the Exeter outbreak of 1883—and here, as in the case of cotton, the infection must have survived in the fabric for several weeks, if not months. I am inclined to doubt the need for suggesting that the small-pox organism in these cases lives a saprophytic existence in the cotton. Probably even the dampest bales are relatively dry, and the power of small-pox infection to remain on dried articles is well known. In the days of small-pox inoculation the virus used to be sent to country places on threads. Similarly vaccine lymph, when dried on ivory points, will keep a long time; in former days I have known public vaccinators to use points sent to them a year before, and obtain normal results from them. It seems enough to suppose that the small-pox organism in the cotton is in a resting stage, and not in any active condition of saprophytic growth. If it be a fact that an outbreak starting with infection at a cotton mill is usually mild in type, the occurrence is not necessarily to be ascribed to some inherent change in the nature of the virus due to its life on the cotton; it may be due, especially in the case of American cotton, to a mild type of the disease in the country of origin. Administratively, the occurrences described by Dr. Corbin raise some question of regarding small-pox as an occupational disease of a certain section of cotton workers and dealing with it accordingly, though of course it is a liability which any worker can escape by keeping himself protected by re-vaccination. Sometimes the Local Government Board is urged to impose restrictions on passengers arriving in vessels on which small-pox has occurred, or coming from ports where small-pox is known to be prevalent, which are much more drastic than those at present in operation. It is well to remember that even if such restrictions can be justified on their merits they will not affect the risk of importation of small-pox in the ways which the contributions of Dr. Corbin and Dr. Macewen have illustrated.

After some remarks by the PRESIDENT, who drew attention to the fact that the virus of small-pox had a better chance of existence under conditions of dry and cold environment—

Dr. CORBIN replied: The fact, pointed out by the President, that the virus of small-pox retains its vitality better under dry and cold conditions does not exclude the possibility of a saprophytic condition in which moisture and warmth may be necessary. It may, indeed, be a good thing that cotton bales are not dried and cold-stored, as under such conditions the germ might be imported in the bales in a more virulent state than appears to exist under ordinary circumstances. The question asked by Sir Ronald Ross, why the imported disease is, as a rule, very mild, is difficult to answer unless one assumes that the virus is attenuated, or of a mild strain in the first place; the latter suggestion is supported by the probability that most of the infection

which gets to the raw cotton is from mild and missed cases which continue to work in the fields. One or two of the Milnrow cases were severe, possibly owing to the massiveness rather than to the virulence of the infection. It is interesting in these cases that such a large percentage of the operatives engaged in the early processes took small-pox, whereas in the card-room only a small percentage of the operatives were infected. This, I think, points to mass infection by dust in the mixing and blowing rooms, while at the frames infection from piecing is more a matter of chance. There is little doubt that many outbreaks in Lancashire previous to 1908 were due to importation of infection in raw cotton, and the interesting case at Oldham indicates how easily it may be overlooked unless particular attention in the investigation is paid to this possibility; and while one would hesitate to associate this disease with such an important industry, it is gratifying to know that by taking proper precautions in dealing with dust, and by vaccination, the danger may be entirely obviated.

The Occurrence of an Unusual Cough among Weavers of Cotton Cloth.

By Edgar L. Collis, M.B.

A short summary of the processes through which cotton passes before it becomes cloth may help those unfamiliar with the industry to understand the conditions associated with the illness I am about to describe.

Raw cotton, which is imported in large bales, is first dealt with in spinning mills, where it is spun into long threads. This branch of the trade is carried on in factories quite distinct from those where the threads are woven into cloth. The threads are sent out from the spinning mills wound up in the form of small cylinders, known as "kops." At the weaving factory, those threads which are to form the warp or longitudinal threads of the cloth are wound on to big drums, similar in size and shape to the cylinders upon which garden watering hose is kept. During the process of weaving these threads are kept on the stretch and are liable to break. In order to strengthen them, therefore, before they are placed on the loom they are passed through a hot emulsion of starch, china clay and other ingredients; this process is known as sizing or taping. The threads which are to form the weft or cross-threads of the cloth are not sized, but are placed, just as they come from the spinning mill, into the shuttles. For some classes of cloth only sufficient size is added to facilitate weaving; but for others more size is added in order to give weight and firmness to the finished cloth. When heavily sized threads are being woven, artificial humidity, in the form of steam or water-spray, is frequently introduced into the weaving shed in order to prevent the threads from breaking, which they do if they become too dry; and such sheds are known as "humid" sheds. Sheds where no artificial humidity is introduced are known as "dry" sheds.

During 1910 and 1911 an unusual cough appeared among weavers in a Lancashire cotton mill; in the spring of 1911 similar symptoms in a marked form broke out in a mill at Colne, and, after subsiding, recurred in June, 1912. About this latter date the weavers in two weaving sheds in Burnley also were affected, those in one so severely that the shed had eventually to be closed. In 1913, similar symptoms, though less pronounced, occurred at another shed; and during the past

year, 1914, a few cases have occurred at two other mills. In each case the symptoms occurred in a "dry" shed, in which cloth is woven for subsequent bleaching, printing, and dyeing. Such cloth differs in the process of manufacture from other cotton cloth, in two respects: a smaller quantity of size is applied to the warp threads, and antiseptics are omitted from the size. Antiseptics are added to size when cloth which does not pass through any further process is being woven, to prevent the apppearance of mildew on the cloth before it reaches the market. Zinc chloride, which possesses hygroscopic as well as antiseptic properties, is the antiseptic generally used; but dyers object to it because cloth containing it is very liable to take fire when passed over hot rollers, and specify that no chlorides shall be present in the cloth they buy. Size is composed of various ingredients, of which the most important are flour (derived from wheat, sago, or potatoes), tallow, china clay, and water. Cotton threads passed through such a preparation, in the absence of any antiseptic, if not thoroughly dried, form an excellent soil for the growth of moulds. Mildew spores, which are present in any atmosphere, must settle on the threads as they are wound from the sizing machines on to the warp beam. Moreover, to cool and further dry the threads as they pass off the warm cylinders, paddle fans, revolving close to the bottom of the machines, drive air against the threads, to which, if they are damp, any spores in this air will readily adhere. Unsized threads, if not kept dry, may also become mildewed; and, because cotton is sold by weight, a custom has arisen in recent years of "conditioning" kops, which consists in adding moisture to the cotton at the spinning mills. Kops of thread so moistened soon begin to "sweat" and become mildewed. In this way weft as well as warp threads may be affected.

Conditions favourable to the development of mildew appear to have been present wherever the illness has occurred. Thus the outbreak at Colne, in 1911, was preceded by a complaint from the weavers that threads were frequently breaking and impeding work, which was attributed to "dry taping" of the warp threads (the technical term for the process of applying size to warp threads is known as "tape sizing" or "taping"). To remedy this trouble "wet taping" was resorted to—i.e., threads were passed quickly over the drying cylinders of the sizing machine and, while still containing moisture, wound on to the warp beam. When these threads were placed on the looms they showed signs of mildew, and the illness broke out. Dry threads were substituted for the mildewed ones and the symptoms disappeared.

During May, 1912, which was a month of dry weather, the firm thought it advisable to revert to wet taping; in June, however, contrary to expectation, damp weather prevailed, mildew again appeared, and with it the symptoms; when the mildew was got rid of, the symptoms, never so pronounced as in the previous year, disappeared.

The weaving shed at Burnley where the most severe cases occurred contains over 1,000 looms, which are supplied with warp threads prepared on two sizing machines, the threads prepared on one machine going generally to the looms at one end of the shed, and those prepared on the other machine to looms at the other end. The machines are tended by separate men, one of whom, prior to the outbreak, taped wet, while the other taped dry. The threads taped wet became mildewed, and only the operatives working on or near the looms on which these warps were used became affected.

At the other mills where similar symptoms have occurred mildew has also appeared on the threads, in some cases on the warp and in some cases on the weft.

Visible moulds, due to growths of *Penicillium glaucum* or *Aspergillus glaucus*, which are bluish-grey in colour, occur on warp threads from time to time, but have not previously been associated with any affection among the operatives; when, however, the symptoms to be described later occurred, not only was mildew observed on the threads, but also an unpleasant smell was noticed, a smell which was detected on threads showing no visible signs of mildew. Possibly some unusual mildew, of which the growth is similar in colour to cotton thread, was present; and affected weavers have several times referred to the presence of sulphur-coloured dust on the looms.

The above considerations show that the outbreaks have in some way been associated with the presence of mildew.

The first sensation experienced is one of constriction of the chest beneath the sternum; this feeling extends up towards the larynx, and the act of breathing becomes an effort. Next, breathing becomes rapid and rather shallow with occasional deep sighs, and a persistent irritating cough develops. This cough is associated with expectoration of thick yellow or yellowish-green sputum and a sweetish taste in the mouth. Later the difficulty in breathing progresses until it resembles asthma and may prevent the sufferer from sleeping, or arouse him from a disturbed rest fighting for breath, and drive him to throw open the bedroom window for air. Food may be rejected in a spasm of coughing, epistaxis may occur, and the fauces are found red and inflamed. The

individual suffers with general malaise, aching limbs and back, severe headache which is usually frontal, and shivering, ending in perspiration; in a few cases where the temperature has been taken it has been found to be from 100° F. to 102° F. in the evening, but no temperature above normal has been recorded earlier in the day. As the symptoms persist the individual cannot hurry when walking, finds an ordinary slope difficult to climb, and tires rapidly; the appetite becomes affected and weight is lost. Pronounced cases have been deprived of sleep for a week and have been confined to bed, but those less affected have managed to remain at work. In a few cases herpetic eruptions on the face, palpitation, smarting and running of the eyes, and loss of the sense of smell have occurred; but no diarrhœa, jaundice, pigmentation of the skin, high-coloured urine, or symptoms of disorder of the gastro-intestinal canal (characteristics of poisoning by arsenical gases) have been present, and the colour of the mucous membranes has been normal.

Several of the cases have been at first considered by their medical attendants to be early tuberculosis; but the absence of tubercle bacilli from the sputum, and complete recovery in every case following upon absence from work, have negatived this diagnosis. But it is of interest to note that Rolleston states[1] that aspergillosis may clinically resemble chronic pulmonary tuberculosis, but differs in tending to undergo a gradual and spontaneous cure. Arkle and Hinds say[2] that in this condition hæmoptysis is infrequent, that there is loss of flesh and strength, that frequent cough and severe dyspnœa come on in attacks at night, and that cases which appear like chronic phthisis may be marked by attacks of pseudo-asthma; this description closely resembles that given above.

Moulds may cause illness in two ways:—

(1) In certain circumstances mildew growths liberate injurious gases, as, for example, di-ethyl-arsine, which is formed when *Penicillium brevicaule* and some other mildews grow on arsenical wall-paper. In favour of this view of the recent outbreak are the facts that (*a*) the symptoms appeared with great rapidity, for, when the operatives resumed work at Burnley after a fortnight's absence, several of them were seen to be suffering at eleven o'clock in the morning, an onset so rapid as to leave but little time for an incubation period;

[1] "System of Medicine," edited by Sir Clifford Allbutt, 1898, v, pp. 257-263.

[2] *Ibid.*, p. 260.

(*b*) a definite smell was noticed on some warps; and (*c*) the operatives at Burnley who worked near two exhaust fans, which would draw any gas towards them, placed in the roof, were more affected than those farther off. On the other hand, no substance from which mildews are known to form injurious gases was found either by Dr. Ritchings, of Burnley, who was called in to investigate the matter, or at the Government Laboratory, in the size used, in its ingredients, or in the urine of a patient.

(2) Spores of some mildews are known to cause illness after inhalation, and although mildewed threads are damper than usual, and so less liable to disseminate dust, yet spores detached from these threads might readily be drawn into the air-passages during the manipulations of weaving.

Aspergillus fumigatus has been described[1] as causing, in Paris, a rare trade disease, aspergillosis, among (1) persons who feed pigeons artificially, and among (2) hair sorters who employ flour of rye to enable them to separate the hairs more readily, and who may inhale dust containing the aspergillus of rye flour.

What special mildew has been the cause in the cotton mills I cannot say, because, although various forms of mildew were found (in the flour used) by the experts called in by the occupiers, no opportunity for pathological investigation has arisen. Mildews of various kinds—blue, grey, and red—are well known in the cotton trade, but knowledge as to pathogenic properties of mildews is scarce.

The occurrence of these cases is, I think, of interest; they exhibit a distinct and definite train of symptoms, always associated with an occupational environment suggestive of the cause. Yet medical men attending them, each seeing only one or two cases, have not considered them unusual, but have passed them by as ordinary cases of bronchitis, asthma, or even incipient phthisis.

[1] *Ibid.*, p. 258.

Section of Epidemiology and State Medicine.

June 4, 1915.

Dr. W. H. Hamer, Vice-President of the Section, in the Chair.

The Statistics of Anti-typhoid and Anti-cholera Inoculations, and the Interpretation of such Statistics in general.

By Mr. Major Greenwood, jun., and Mr. G. Udny Yule.

Introduction.

Hardly any subjects within the range of preventive medicine are of more immediate importance than the methods of prophylaxis which ought to be adopted with respect to typhoid fever and cholera.

Typhoid fever has already been responsible for much illness and many deaths in nearly all the armies on active service, while cholera has taken toll of one at least of our enemies and one of our allies. Further, our troops are now fighting in a part of Europe and Asia which has always been a favourable soil for the development of epidemic cholera and was recently the scene of outbreaks among troops actually engaged in the present war.

Amongst the measures of prophylaxis which need to be discussed, that of preventive inoculation is clearly of exceptional interest, since such other hygienic precautions as experience would sanction are difficult, if not impossible, to carry out under service conditions. The expediency of inoculating troops against typhoid has been keenly debated, particularly since the outbreak of war, and the verdict of the medical profession has been almost unanimous. We have no evidence to bring forward which has not already done duty in the lay or scientific press within the last few months, and the remarks we shall have to make upon this branch of our subject will be brief. The data relating to cholera, on the other hand, have not attracted so much public attention, although, for the reasons just adduced, their importance is

great, and we shall be obliged to devote a good deal of space to their consideration. We have also been led to discuss various theoretical problems which might have been thought more suitable to the pages of a purely statistical journal. We are, however, satisfied that these questions of method ought to be studied in connexion with the practical problems from which they originate, and we hope that the arrangement and subject-matter will be found to conform to the logical necessities of the case.

In Section I we shall lay down the conditions which are, in our opinion, necessary to secure data from which valid statistical conclusions can be drawn and shall deal with the question of errors due to the fluctuations of sampling.

In Section II the available data are examined from the point of view defined in the first section.

In Section III we develop a statistical theory of the way in which immunization results can be interpreted.

In Section IV we consider the application of certain statistical methods of measuring association to data of the present kind.

Section I: The Conditions necessary for Valid Inference.

In the vast majority of cases one is provided with data arranged, or capable of being arranged, in the following manner: (*a*) A number of persons who have been inoculated against a certain disease and have not contracted the disease when exposed to risk; (*b*) a number of persons who have been inoculated and have contracted the disease; (*c*) a number of persons who have not been inoculated and have not contracted the disease; (*d*) a number of persons who have not been inoculated and have contracted the disease. If the statistics refer not to incidence but to fatality, the arrangement is the same, substituting "died of" or "recovered from" for "contracted the disease" or "did not contract the disease."

Given such data, the two following questions are those one is asked to answer: (1) Is there a *significant* difference between the attack (or fatality) rates of the two classes--i.e., is the observed difference greater than we could fairly attribute to the action of chance? (2) Assuming that the answer to (1) is in the affirmative, what is the degree of association between being inoculated and escaping from or being affected to a less degree by the disease, and how can we compare the result of one trial with that of another from this point of view?

It is not necessary at the moment to inquire whether these questions can be satisfactorily answered at all; whether they can be so or not is a question, principally but not entirely, of method, of statistical method, and its discussion presupposes the removal of certain material difficulties, assumes, in fact, that the four categories are homogeneous except as regards inoculation and death or attack. All the following conditions should be fulfilled by the data:—

(1) The persons must be, *in all material respects*, alike. The inoculated subjects must not differ in age, sex, social or racial constitution from the uninoculated subjects, unless it can be shown, on other grounds, that such differences do not affect the liability to contract the disease or, when one is investigating fatality, to die from it if contracted. We have italicized the proviso as to the similarity being material, for cases may be adduced in which although the persons are not *absolutely* alike they may be so in all material respects. For instance, in the case of typhoid, if personal attention to cleanliness under the ordinary conditions of army life can seriously reduce the liability to contract the disease, it might be argued that volunteers for inoculation are not homogeneous with uninoculated men, because the former, in virtue of their willingness to submit to inoculation, have proved themselves to be more careful than their comrades, and consequently would be expected to have less typhoid than the latter quite apart from inoculation. If, however, the ætiology of typhoid is such that no practicable care on the part of the individual can substantially reduce his risk of contracting the disease, then this particular heterogeneity is not material and can be disregarded.

In most cases, however, we have no such independent evidence, and any want of homogeneity in the above particulars must be deemed *prima facie* material.

(2) The effective exposure to the disease must be identical in the case of inoculated and uninoculated persons.

This condition, although clearly vital, has been often disregarded by medical writers. Evidently, if the uninoculated have been longer at risk than the inoculated, as would be the case if inoculation is carried on throughout an epidemic, and the statistics are compiled from the totals of inoculated and uninoculated at the end of the experience, the condition is not fulfilled. Neither is it fulfilled, even though the time of exposure is the same, if the conditions of exposure are not the same—e.g., if the records of inoculated persons are taken in one year and those of the uninoculated in another year. We could only

regard the heterogeneity of the second case as immaterial if a proof were furnished that the incidence (or fatality) of the disease is invariable from year to year; no such proof can be provided in the case of either typhoid or cholera.

(3) The criteria of the fact of inoculation and of the fact of the disease having occurred must be independent.

That the classification into inoculated and uninoculated must not be influenced in doubtful cases by knowledge of whether the disease had or had not been contracted may seem so obvious a stipulation that we need not trouble to set it out. In those cases in which the bacteriology of the disease is unequivocal and only a bacteriological diagnosis is admitted, while the fact of inoculation or its absence is recorded without fail in the case of every person exposed to risk, fulfilment of the condition only requires *bona fides* on the part of the observers. But when, as may quite frequently happen, doubts legitimately arise as to whether a given person had or had not been inoculated and the collection of records was entrusted to subordinates, convinced on other grounds of the efficacy of inoculation, it is easy to see how seriously biased statistics may be prepared without any evil intent. Such considerations no doubt afford any justification it may possess to Sticker's remark that "it is an open secret in India, as elsewhere, that for inoculation statistics the most favourable examples are always chosen" (Sticker, 1912, p. 313).

It will sometimes happen that the data themselves afford means of coming to a conclusion as to their material accuracy in the sense of the above remarks. This is so when we are provided with records of attack- or fatality-rates for inoculated and uninoculated persons in the case of several epidemics of the same disease. We shall return to this point in a later section, but may remark here that, upon almost any plausible theory of immunization, the attack- or fatality-rates of both classes in a series of epidemics of varying severity should be highly correlated. To take a simple illustration, the work of Dorset and others has shown that the larger the dose of virulent blood injected the larger must be the quantity of hyper-immune serum employed to protect a pig from swine fever, and that one of the difficulties of the method of simultaneous inoculation is to adjust the balance accurately. From this it follows that were the same dose of hyper-immune serum alone employed in different epidemics of the naturally occurring disease the fatality must vary among the inoculated in the same sense as among the uninoculated. This reasoning

is, we think, generally applicable, and if we find that over a series of epidemics the case- or fatality-rates of inoculated and uninoculated persons are not highly correlated, the discovery is evidence that the data are unreliable. Examples will be provided hereafter.

All the preceding conditions must be fulfilled to provide an accurate material basis for conclusions. But a further condition is necessary—namely, that the number of observations must be sufficient. We have, therefore, to deal with the first of our statistical problems —viz., to provide a criterion of the probability that any difference between the incidence- or fatality-rates of the inoculated and uninoculated persons actually found in the data should be deemed significant of an organic distinction between the two classes.

The criterion which has been adopted by most statisticians was introduced by Professor Karl Pearson in 1900 and was developed by him in the following way: If we know *a priori* that a table containing n "cells" should include $m_1, m_2, m_3 \ldots\ldots m_n$, observations in the respective cells, then, if the standard errors of sampling and the correlation in errors of the various cells are of a certain form, the probability that fluctuations of sampling alone would give rise to a system of deviations $m_1 - m_1'$, $m_2 - m_2' \ldots\ldots m_n - m_n'$ where $m_1', m_2', m_3' \ldots\ldots m_n'$ are the actually observed contents of the "cells," or to any system of wider deviations, is a function of $\mathbf{S}_1^n \frac{(m_n - m_n')^2}{m_n} = \chi^2$. The probabilities corresponding to different values of this function for various numbers of subdivisions or "cells" of the table have been computed by Elderton and are published in "Tables for Statisticians and Biometricians" (Cambridge University Press, 1914). Thus, if the number of subdivisions be 4 and χ^2 computed as above on the basis of some assumed theoretical distribution be 1, then the chance (P) is about 8 in 10 that errors of sampling would lead to as great a discrepancy as or a greater discrepancy than that actually observed between theory and observation. Had χ^2 been 2 the chance would have been about 57 in 100, had it been 8, the chance would have been about 46 in 1,000, and the probability goes on diminishing as χ^2 increases, being less than one in a thousand for $\chi^2 = 17$, and only one in a million for $\chi^2 = 30$.

The application of this test in the present case presupposes a theoretical distribution with which we compare our observed distribution, and, since we wish to determine the probability that any given arrangement has been brought about not by a fundamental connexion between the fact of inoculation and the fact of recovering or escaping

from the disease but by the operation of chance, we naturally assume as the theoretical law that the events are independent. In other words, if the chance of being inoculated is α and of escaping from the disease β, then in N trials we should expect to find N $\alpha\,\beta$ persons who have both been inoculated and escaped the disease; three similar expressions will give us the theoretical numbers falling within each of the other three subdivisions of the data, and then, since we know the actually observed frequencies within the four "cells," χ^2 can be computed and the required probability estimated with the aid of Elderton's table. It must be noticed that the application of this test to inoculation data is based upon an assumption. We do not, in fact, know the true values of α and β and must replace them by the observed ratios of the number of inoculated persons to the total frequency and the number of cases of disease (or deaths) to the total. This is not strictly correct, from the point of view of the general theory, but when we are dealing with such distributions as those actually in question, there is, perhaps, no more impropriety in making the assumption than in following the same course when we compute errors of simple sampling in the ordinary way.

A further point is worthy of remark. In our subsequent discussion we shall frequently compare the two ratios $\frac{\text{inoculated attacked}}{\text{all inoculated}}$ and $\frac{\text{uninoculated attacked}}{\text{all uninoculated}}$ which we may denote by p_1 and p_2. It might therefore be asked, why we should not adopt as our criterion of significance the ratio of $p_1 - p_2$ to its standard error, counting as significant all differences greater than some assigned multiple of the standard error. It will be found that if this plan is adopted deviations which, judged by the χ^2 test, are not improbable are much less likely to occur as the result of random sampling. This divergence between the results of the two tests is at first sight rather surprising and is not due to neglect of the correlation in errors between the subgroup frequencies. If the standard errors of p_1 and p_2 are worked out from first principles it will be found that the ordinary binomial form results, and that there is no correlation in errors between p_1 and p_2.

The explanation is, we think, as follows: The total number of distributions into, for example, four "cells" of n things which differ from the expected distribution by more than a certain margin is greater than the number of those which fulfil the further condition that the difference between the ratios $\frac{b}{a+b}$ and $\frac{d}{c+d}$ (p_1 and p_2 of our previous remarks, a being the number of inoculated who recovered, b the

number of inoculated who died, d and c corresponding frequencies of uninoculated) shall exceed a certain magnitude. The result will be that the probability of any arrangement having arisen from random sampling, or of any less probable arrangement, will be greater when estimated by the χ^2 method than when the other test is applied. It can, of course, be urged that we are really only concerned with the probability that chance might give rise to those arrangements which exhibit a difference between $\frac{b}{a+b}$ and $\frac{d}{c+d}$, and we are not convinced that the objection is invalid. We think, however, that the point is not free from difficulty and merits further consideration from the theoretical side, to which we have no space in the present paper to devote. Again, it is evident from our preceding remarks that any distribution deemed improbable on the basis of the χ^2 test will, *a fortiori*, be rejected by the other test. Consequently, in judging data by the χ^2 test we shall certainly not attach significance to results which might be the mere effects of random sampling and shall err on the side of caution.

Section II: Examination of the Available Data.

We shall now consider the statistics actually available for our inquiry, beginning with—

(*a*) *Anti-typhoid Inoculation.*

The only statistics of importance are still those compiled by the Anti-typhoid Inoculation Committee; the information afforded by the authorities with respect to inoculation of the troops in France and Belgium is far too meagre to be noticed here. The material criticisms to which the Committee's data were exposed are: (1) The inoculated men volunteered, they were not selected at random; (2) paratyphoid cases were excluded. With respect to the second point, we, having the fear of Dr. Hamer before our eyes, do not venture to express a personal opinion, but we may submit that the propriety of the exclusion has been admitted by all bacteriologists. We have already noticed (1) and need merely add that a study of the special conditions under which the test units were placed and of the ætiology of typhoid in the regiments confirms us in holding that the heterogeneity present was immaterial. A statistical question which arises is as to whether we should class as inoculated those who were so at the date of the last return made or only those actually inoculated at the time of arrival on the foreign

station. In the former case, we may be exaggerating the number of men who were inoculated during the whole period of exposure to infection, in the latter we shall underestimate it because many inoculations were done shortly after arrival.

The statistics have been arranged in both ways (Tables I and II) and the values of χ^2 computed. Both tables exhibit divergences from the distributions which might be expected if inoculation were without influence upon the chance of contracting typhoid, and these divergences are very unlikely to occur as errors of sampling. We may remark that if Professor Pearson's coefficient for a fourfold table be deemed an appropriate measure (see our discussion of this matter in Section IV), it will be found that the same conclusion emerges. This coefficient for Table I is 0·39 and for Table II 0·24, and each coefficient is several times the size of its standard error.

TABLE I.—ANTI-TYPHOID COMMITTEE'S DATA.

First arrangement.

	Not attacked	Attacked	Total
Inoculated	10,322 ...	56 ...	10,378
Not inoculated	8,664 ...	272 ...	8,936
Total	18,986 ...	328 ...	19,314

$\chi^2 = 180{\cdot}38$. P = less than 0·0001.

TABLE II.—ANTI-TYPHOID COMMITTEE'S DATA.

Second arrangement.

	Not attacked	Attacked	Total
Inoculated	6,759 ...	56 ...	6,815
Not inoculated	11,396 ...	272 ...	11,668
Total	18,155 ...	328 ...	18,483

$\chi^2 = 56{\cdot}23$. P = less than 0·0001.

In view of the fact that the risk of serious injury resulting from inoculation is inappreciable and that even transitory discomfort is uncommon, provided the precautions as to rest and abstinence from alcohol immediately after the operation advised by the Committee are taken, the case in favour of anti-typhoid inoculation as a practical measure is very strong. We regret, however, that we have no more evidence to bring forward.

(*b*) *Anti-cholera Inoculation.*

A general account of the earlier attempts to confer immunity from cholera by inoculation will be found in Sticker's treatise (1912, pp. 307, &c.), and a fuller description in Haffkine's recent monograph. These accounts supplement one another, since Sticker views the subject from the standpoint of a convinced opponent of accepted teaching as to the pathology and epidemiology of cholera, while Haffkine writes with the natural enthusiasm of a pioneer. Haffkine's published experience relates mainly to the years 1894-99 and does not cover epidemics among troops on active service. More recent experience comparable with that of Haffkine is recorded by Nijland in the case of the Dutch East Indies and by Murata, who deals with some outbreaks in Japan.

The epidemiological history of cholera which is of most importance at the present time is that of the last Balkan War. This campaign called forth papers describing the conditions in the combatant armies which will repay examination. The Turks do not appear to have resorted to inoculation, indeed, so far as we can judge, their sanitary organisation was in all respects chaotic. Cholera broke out in their army after its defeat at and flight from Lule Burgas (October 30-31, 1912), (Simond, Pasteur Vallery-Radot, Kiamil Bey, Asseo). The ravages of the disease were enormous; we have no trustworthy statistics, and accuracy of statement is not a *very* strong point of the Turks, but some observers put the number of deaths within a short period as high as 30,000. If this statement is even approximately correct, the gravity of the epidemic from every point of view need not be emphasized. This was less than three years ago, and our own troops are fighting in the same part of Europe and opposed by the very army which suffered this calamity. On the other hand, we have accounts of cholera in the Greek army which paint a very different picture (Savas, Cardamatis, Moutouses). It will be convenient to defer consideration of this outbreak until we come to the analysis of the statistical data derived from it.

We shall now examine the evidence provided by the various observers, beginning with that of Haffkine.

Haffkine's Data.

The eight sets of data extracted from Haffkine's treatise relate to the period 1894-99, and the practice of this epoch differed essentially from that now in vogue, since living cultures were employed. Two

vaccines were used in some of the inoculations. Of these, vaccine "II" was a living virus brought by successive passages through guinea-pigs to a uniform and stable degree of virulence. Vaccine "I" was an attenuated virus which had been found to protect guinea-pigs from the local lesion observed to follow the inoculation of vaccine "II." Haffkine, however, having discovered that no such lesion was produced in man, discontinued the use of vaccine "I" after 1896.

TABLE III.—CALCUTTA, 1894-96. PERSONS EXPOSED FROM FIFTH TO FOUR HUNDRED AND SIXTEENTH DAY AFTER INOCULATION.

	Not attacked	Attacked	Total
Inoculated	276 ...	3 ...	279
Not inoculated	473 ...	66 ...	539
Total	749 ...	69 ...	818

$\chi^2 = 29{\cdot}70$. P = less than 0·0001.

TABLE IV.—1ST BATTALION EAST LANCASHIRE REGIMENT, 1894. VACCINES I AND II. EXPOSURE FOURTEEN MONTHS AFTER INOCULATION.

	Not attacked	Attacked	Total
Inoculated	115 ...	18 ...	133
Not inoculated	520 ...	120 ...	640
Total	635 ...	138 ...	773

$\chi^2 = 2{\cdot}04$. P = 0·5652.

TABLE V.—BRITISH TROOPS AT CAWNPORE, 1894. EXPOSURE THIRTEEN MONTHS AFTER INOCULATION.

	Not attacked	Attacked	Total
Inoculated	75 ...	0 ...	75
Not inoculated	778 ...	19 ...	797
Total	853 ...	19 ...	872

$\chi^2 = 1{\cdot}83$. P = 0·6113.

TABLE VI.—2ND BATTALION EAST MANCHESTER REGIMENT AT DINAPORE AND CAMP BETA, 1894.

	Not attacked	Attacked	Total
Inoculated	193 ...	0 ...	193
Not inoculated	723 ...	6 ...	729
Total	916 ...	6 ...	922

$\chi^2 = 1{\cdot}60$. P = 0·6639.

TABLE VII.—GYA JAIL, 1894. INOCULATIONS DURING AN EPIDEMIC. VACCINES I AND II.

	Not attacked	Attacked	Total
Inoculated	200 ...	8 ...	208
Not inoculated	182 ...	20 ...	202
Total	382 ...	28 ...	410

$\chi^2 = 5{\cdot}90$. P = 0·1176.

TABLE VIII.—DURBHANGA JAIL, 1896. INOCULATIONS DURING AN EPIDEMIC. VACCINE II.

	Not attacked	Attacked	Total
Inoculated	105	5	110
Not inoculated	88	11	99
Total	193	16	209

$\chi^2 = 3{\cdot}18$. P = 0·3682.

TABLE IX.—MARGHERITA COOLIES. INOCULATIONS DURING AN EPIDEMIC, 1895. VACCINES I AND II.

	Not attacked	Attacked	Total
Inoculated	192	4	196
Not inoculated	113	34	147
Total	305	38	343

$\chi^2 = 37{\cdot}92$. P = less than 0·0001.

TABLE X.—CACHAR TEA ESTATES COOLIES, 1895-96. VACCINE II.

	Not attacked	Attacked	Total
Inoculated	5,751	27	5,778
Not inoculated	6,351	198	6,549
Total	12,102	225	12,327

$\chi^2 = 111{\cdot}92$. P = less than 0·0001.

Since this particular method is no longer employed, any lengthy comment is unnecessary. It will be seen that three only of the trials, Calcutta, 1894, Margherita, and Cachar, gave values of χ^2 greater than six for which P = 0·11; of these Cachar furnishes the largest value of χ^2. The Cachar results, which were obtained by Powell, will be reconsidered in the section devoted to the latter's publications; while recognizing that we have not sufficient evidence to permit of forming a complete judgment as to whether the inoculated and uninoculated be truly comparable in other respects, we shall not be going too far if we assert that these three results establish a presumption in favour of the value of Haffkine's inoculation.

Powell's Data.

Powell used a twenty-five to thirty-six hours' culture of the vibrio; sterilized water was added to one-third height of the agar, the growth was washed off and suspended in water by shaking; the dose used for an adult was c.mm. The whole of Powell's results to December, 1899, are quoted by Haffkine and summarized above under the heading Cachar Tea Estates. In order to see whether there was any marked heterogeneity, so far as the statistics could reveal it, between the

different estates we have analysed some of the separate returns. It will be seen that in most cases they are not based upon large enough numbers to yield satisfactory results, so that we should rest our conclusions as to Powell's evidence upon the Cachar total. A perusal of Powell's paper rather inclines one to believe that his material is fairly homogeneous, so that the value of the result in the Cachar material is increased. It may be noted that in this case Professor Pearson's normal coefficient is large ($0{\cdot}42 \pm 0{\cdot}02$).

TABLE XI.—KARKURI.

	Not attacked	Attacked	Total
Inoculated	409 ...	3 ...	412
Not inoculated	174 ...	8 ...	182
Total	583 ...	11 ...	594

$\chi^2 = 9{\cdot}34$. P = 0·0256.

TABLE XII.—KALAIN.

	Not attacked	Attacked	Total
Inoculated	1,625 ...	5 ...	1,630
Not inoculated	1,022 ...	11 ...	1,033
Total	2,647 ...	16 ...	2,663

$\chi^2 = 6{\cdot}08$. P = 0·1084.

TABLE XIII.—KALAINCHERRA.

	Not attacked	Attacked	Total
Inoculated	191 ...	0 ...	191
Not inoculated	608 ...	8 ...	616
Total	799 ...	8 ...	807

$\chi^2 = 2{\cdot}51$. P = 0·4802.

TABLE XIV.—DEGUBBER.

	Not attacked	Attacked	Total
Inoculated	431 ..	5 ...	436
Not inoculated	291 ...	9 ...	300
Total	722	14	736

$\chi^2 = 3{\cdot}27$. P = 0·3565.

TABLE XV.—DUNA.

	Not attacked	Attacked	Total
Inoculated	54 ...	5 ...	59
Not inoculated	46 ...	15 ...	61
Total	100 ...	20 ...	120

$\chi^2 = 5{\cdot}61$. P = 0·1351.

TABLE XVI.—RIVER.

	Not attacked	Attacked	Total
Inoculated	212 ...	1 ...	213
Not inoculated	42 ...	1 ...	43
Total	254	2	256

$\chi^2 = 1{\cdot}59$. P = 0·6662.

Murata's Data.

Murata employed a devitalized vaccine, heat was the agent of destruction, a temperature of 60° C. The dose was 1 c.c. containing 2 mg. of bacteria, and apparently only one inoculation was given. Murata seems to have some doubt as to the homogeneity of his statistics (p. 607), and he remarks that the dose should probably have been not 1 but 2 c.c. In view of the fact that the dosage, assuming that but one inoculation was given, was, judged by recent standards, insufficient, and that the temperature of sterilization exceeds that now employed, a point which is now known to be of great importance in the case of typhoid immunization, we should hardly anticipate very satisfactory results. Statistical analysis confirms this view. Only two cases in the series survive the χ^2 test, and of these one shows a *lower* proportion of attacks among the uninoculated. We think, in view of the facts recited, this series proves nothing one way or the other.

TABLE XVII.—KOOBE.

	Not attacked	Attacked	Total
Inoculated	14,939 ...	20 ...	14,959
Not inoculated	243,328 ...	753 ...	244,081
Total	258,267 ...	773 ...	259,040

$\chi^2 = 14{\cdot}48$. P = 0·0024.

TABLE XVIII.—HIMEJI.

	Not attacked	Attacked	Total
Inoculated	2,596 ...	0 ...	2,596
Not inoculated	28,680 ...	15 ...	28,695
Total	31,276 ...	15 ...	31,291

$\chi^2 = 1{\cdot}36$. P = 0·7189.

TABLE XIX.—KAWABE.

	Not attacked	Attacked	Total
Inoculated	8,135 ...	7 ...	8,142
Not inoculated	66,117 ...	88 ...	66,205
Total	74,252 ...	95 ...	74,347

$\chi^2 = 1{\cdot}25$. P = 0·7440.

TABLE XX.—MUKO.

	Not attacked	Attacked	Total
Inoculated	2,440	0	2,440
Not inoculated	80,713	62	80,775
Total	83,153	62	83,215

$\chi^2 = 1{\cdot}87$. P = 0·6022.

TABLE XXI.—AKASKI.

	Not attacked	Attacked	Total
Inoculated	9,297	3	9,300
Not inoculated	60,074	52	60,126
Total	69,371	55	69,426

$\chi^2 = 2{\cdot}99$. P = 0·3934.

TABLE XXII.—KAKO.

	Not attacked	Attacked	Total
Inoculated	2,729	1	2,730
Not inoculated	54,885	10	54,895
Total	57,614	11	57,625

$\chi^2 = 0{\cdot}46$. P = 0·9086.

TABLE XXIII.—INNAMI.

	Not attacked	Attacked	Total
Inoculated	657	0	657
Not inoculated	49,944	8	49,952
Total	50,601	8	50,609

$\chi^2 = 0{\cdot}11$. P = 0·9781.

TABLE XXIV.—SHIKAMA.

	Not attacked	Attacked	Total
Inoculated	3,098	2	3,100
Not inoculated	90,540	48	90,588
Total	93,638	50	93,688

$\chi^2 = 0{\cdot}08$. P = 0·9841.

TABLE XXV.—IBO.

	Not attacked	Attacked	Total
Inoculated	9,587	3	9,590
Not inoculated	86,032	1	86,033
Total	95,619	4	95,623

$\chi^2 = 18{\cdot}71$. P = 0·0003.

TABLE XXVI.—HIGAMI.

	Not attacked	Attacked	Total
Inoculated	3,173	0	3,173
Not inoculated	74,471	1	74,472
Total	77,644	1	77,645

$\chi^2 = 0{\cdot}04$. P = 0·9921.

TABLE XXVII.—TSUNA.

	Not attacked	Attacked	Total
Inoculated	19,567	11	19,578
Not inoculated	99,414	49	99,463
Total	118,981	60	119,041

$\chi^2 = 0{\cdot}16$. P = 0·9682.

Nijland's Data.

These data refer to both native and European inhabitants of the Dutch colonies. The author explicitly draws attention to the uncertain value of the statistics of the natives (1911, pp. 476-77); he does not give details of the immunizing process adopted, but it seems probable that one injection of a living vaccine was employed. As will be seen from the tables the population figures are often round numbers and, as is clear from the author's account, must be only approximations. In most of the tables we have placed cases occurring within a few days of inoculation to the credit of the inoculated group, but in the tables of Europeans, 1911-12, we have accepted the author's classification, which, for instance, transfers a case (1912) in an inoculated person, beginning only two days after inoculation, to the uninoculated class. It will be seen that the majority of the tables yield values of χ^2 inconsistent with the hypothesis that the differences between the groups of inoculated and uninoculated persons are attributable to chance. The general sense of Nijland's data is clearly favourable to inoculation, but his paper gives us little confidence as to the worth of the native statistics, while those compiled for Europeans are not accompanied by details as to age, sex, and social class.

TABLE XXVIII.—SINGARADJA.

	Not attacked	Attacked	Total
Inoculated	4,087	5	4,092
Not inoculated	113,856	1,144	115,000
Total	117,943	1,149	119,092

$\chi^2 = 31{\cdot}49$. P = less than 0·0001.

TABLE XXIX.—SEMARANG.

	Not attacked	Attacked	Total
Inoculated ...	8,332	8	8,340
Not inoculated	84,444	556	85,000
Total	92,776	564	93,340

$\chi^2 = 39{\cdot}40$. P = less than 0·0001.

TABLE XXX.—DENPASSER.

	Not attacked	Attacked	Total
Inoculated ...	1,730	3	1,733
Not inoculated	153,549	451	154,000
Total	155,279	454	155,733

$\chi^2 = 0{\cdot}85$. P = 0·8311.

TABLE XXXI.—SELONG.

	Not attacked	Attacked	Total
Inoculated ...	4,870	5	4,875
Not inoculated	153,096	904	154,000
Total	157,966	909	158,875

$\chi^2 = 19{\cdot}50$. P = 0·0002.

TABLE XXXII.—EUROPEANS OF SEMARANG.

	Not attacked	Attacked	Total
Inoculated ...	2,450	0	2,450
Not inoculated	2,638	38	2,676
Total	5,088	38	5,126

$\chi^2 = 35{\cdot}05$. P = less than 0·0001.

TABLE XXXIII.—EUROPEANS IN SOERAKARTA.

	Not attacked	Attacked	Total
Inoculated ...	620	2	622
Not inoculated	814	13	827
Total	1,434	15	1,449

$\chi^2 = 5{\cdot}42$. P = 0·1465.

TABLE XXXIV.—EUROPEANS IN JAVA, 1910-11.

	Not attacked	Attacked	Total
Inoculated ...	3,999	1	4,000
Not inoculated	5,942	58	6,000
Total	9,941	59	10,000

$\chi^2 = 36{\cdot}28$. P = less than 0·0001.

TABLE XXXV.—EUROPEANS IN JAVA, 1912.

	Not attacked	Attacked	Total
Inoculated ...	6,999	1	7,000
Not inoculated	3,687	13	3,700
Total	10,686	14	10,700

$\chi^2 = 21{\cdot}05$. P = 0·0001.

Zabolotny's Data.

This author furnishes us with an interesting account of epidemic cholera in Russia during 1907-08, in which years the disease was widely prevalent. In the city of Petrograd the disease became epidemic in the month of August, 1908; during September of that year the maximum number of cases reached 419 per day, the total for the month being 6,799. By the middle of April, 1909, there were 10,311 cases with 4,006 deaths. The number of persons inoculated in Petrograd during the 1908 epidemic is said to have been more than 15,000, among whom twelve cases only occurred. The total population of the city was about a million and a half. During 1907 more than 4,000 persons were inoculated in the province of Astrachan, more than half of these twice, and the attack-rate was 5 per mille. A comparison is instituted between the inoculated and uninoculated employees of Nobel frères in the city of Tzaritzine (in 1908). The figures are: Of 590 inoculated one was attacked, of 2,390 not inoculated eighteen. These, and other results cited, certainly suggest that the inoculations were of benefit, but we are of opinion that Zabolotny's figures do not lend themselves to exact analysis. As we shall have occasion to demonstrate, à propos of the statistics relating to the Greek Army, inoculation returns compiled during the course of an epidemic are very difficult to interpret unless we have information as to the period of exposure to risk of those persons not inoculated before the disease appeared, but subsequently so protected. The information provided in Zabolotny's paper is insufficient to enable us to avoid the pitfalls, and we cannot, therefore, use his data.

Savas's Data.

The data now to be discussed are of such importance and are so certain to be quoted by all writers on the subject, that it will be necessary to consider them very carefully. Savas's experience relates to the Greek Army during the second Balkan War and the essential facts of the situation are as follows: During the first Balkan War, that between the Allied States and Turkey, no cases of cholera occurred in the Greek Army. As we have seen, however, there was a murderous outbreak among the Turkish troops, and the Bulgarians became infected before and during the siege of the Chataldja lines. From here they carried the infection into Thrace and Eastern Macedonia, territories containing many Greek inhabitants. Fugitive Greeks are assumed to

have conveyed the seeds of infection to within the Greek lines, the first case occurring May 11-24, 1913. At the outbreak of war the exigencies of the service prevented the complete inoculation of the Greek Army and the process was not, in fact, completed until the cessation of hostilities in July. While on the march, the troops drank water polluted with Bulgarian corpses, and civilian fugitives spread the disease somewhat widely through the towns and villages of Macedonia. Not only was there pollution of the water, but flies were abundant and it was noticed that when the troops were encamped at an altitude of more than 1,000 metres, where the streams were unpolluted and flies rare, the cases were rarer. As to the actual course of the epidemic, we are informed that the first case occurred in May, that in July the epidemic declined rapidly and that it came to an end early in October; weekly or monthly returns are not provided. Including cases among the inhabitants of Macedonia, it is estimated that there were from 5,200 to 5,300 attacks with 1,665 to 1,700 deaths. Among the troops there were 2,503 cases, with 515[1] deaths (it will be remembered that in the Turkish Army the deaths during a shorter period were estimated at some 30,000, but the estimate is uncertain and the numbers exposed to risk probably much larger).

From Savas's account, as well as that of his assistant Moutouses, it is plain that the sanitary organization of the Army was of a high order; efforts were made, apparently with success, to separate cholera patients from the wounded practically in the fighting line, and field laboratories were available for deciding the diagnosis. Much importance is assigned to the fact that the control of both civil and military patients was in the same hands.

The history of the inoculations is as follows: They were commenced about two months before the outbreak of war, and all the sanitary division had been inoculated before cholera appeared. The first and seventh army divisions were inoculated after the first case (that in May) had been diagnosed, and the vaccination of the whole army was complete at the end of July or the beginning of August; in all, about 150,000 men were inoculated. In addition a very large number of civilians in Salonika, Macedonia and Old Greece, some 500,000, were inoculated. The material for inoculation was derived from a culture of the vibrio isolated from the fæces of a Bulgarian, its virulence being exalted by

[1] These are the cases returned from the cholera ambulances and military hospital, but it seems probable that some were civilians, for in another table only 2,192 are allotted to the army divisions.

passage through guinea-pigs and maintained constant during the whole period of preparation. A bacillary emulsion containing 4 mg. of bacteria per cubic centimetre was employed for inoculation, it was sterilized at 56° C. and 5 per 1,000 carbolic was subsequently added. The dosage was ½ c.c. for adults, followed eight days later by 1 c.c. The inoculations were rarely attended by complications, but in a few cases fulminating cholera developed within a few hours of the first inoculation. Savas is disposed to regard this as evidence of the reality of a negative phase.

TABLE XXXVI.—MORBIDITY AND VACCINATION IN THE GREEK ARMY.

PARTICULARS AS TO VACCINATION					CHOLERA CASES				
Division	Original strength	Vaccinated once	Vaccinated twice	Not vaccinated	Vaccinated once	Vaccinated twice	Not vaccinated	Doubtful	Total
I	16,259	993	11,526	3,740	48	56	31	—	135
II	12,489	66	22,199 [12,199]	174	66	23	174	96	359
III	11,922	3,138	8,472	312	147	260	48	—	455
IV	14,822	3,306	8,397	3,119	36	20	96	—	152
V	10,859	3,010	7,819	30	90	83	19	—	192
VI	11,081	3,690	6,182	1,209	36	39	119	—	194
VII	13,500	250 [258]	13,192	50	62	64	13	—	139
VIII	9,082	24	9,032	26	7	8	26	—	41
X	11,404	120	10,976	308	120	64	308	—	492
XI	3,435	6	3,426 [3,429]	—	6	27	—	—	33
Totals	114,803	(14,603)? 14,613* [14,611]	(101,221)? 91,224 [91,224]	8,968	618	644	834	96	2,192
Total morbidity	—	—	—	—	42 per 1,000	7 per 1,000	93 per 1,000	—	19 per 1,000

If the figures in the columns are correct the totals should be as marked "?" The figures in brackets are suggested as being more probably correct, and in that case the totals in square brackets will be the right ones. * This total is quoted in the text as 14,411.

In Tables XXXVI and XXXVII we give those of Savas's statistics which we propose to discuss. His statistics of fatalities are not, we think, suitable for analysis on the lines of this paper, being specially open

to the criticisms which we shall make with respect to the incidence records. We have noted on Table XXXVI certain minor discrepancies in the figures and indicated the corrections which seem necessary (*vide infra*).

TABLE XXXVII.—MORBIDITY OF THE SANITARY COMPANIES OF THE DIVISIONS.

Division	Number	Vaccinated once	Vaccinated twice	Not vaccinated	CHOLERA CASES			
					Vaccinated once	Vaccinated twice	Not vaccinated	Total
I	417	11	402	4	—	—	—	—
II	323	—	323	—	—	—	—	—
III	280	—	280	—	—	—	—	—
IV	284	—	284	—	—	—	—	—
V	241	—	241	—	—	4	—	4
VI	393	51	342	—	3	—	—	3
VII	280	—	280	—	—	2	—	2
VIII	308	—	308	—	—	—	—	—
X	371	—	371	—	—	4	—	4
Totals	2,897	62	2,831	4	3	10	—	13
Total morbidity	—	—	—	—	48·38 per 1,000	3·5 per 1,000	—	4·5 per 1,000

Before discussing the statistics themselves, there are some epidemiological points to notice. It is not clear whether we ought to regard the record as of a single outbreak beginning in May and remitting in July or as of a series of separate outbreaks. On the former hypothesis we cannot, in the absence of daily or weekly returns, say whether there was an explosive rise, followed by a decline at first rapid and then slower, when the numbers of fresh cases had fallen to a comparatively low level. Such a course plotted as a curve will give us two unequal areas (the abscissæ being units of time measured from the date of the first case and the ordinates—numbers of cases) if the division is made by a vertical line drawn through the point corresponding to the midpoint in time of the epidemic, and the number of cases during the first period will exceed that of the second period. This is the usual, but not

invariable, state of affairs in epidemic cholera (Table XXXVIII) observed in civil populations, and most of such epidemics have been attributed, on more or less satisfactory grounds, to water-borne infection. Polluted water was certainly one of the sources of infection in the Greek experience, but we have no reason to assume that this was the only or even the most important factor.[1] If, however, we suppose that the

TABLE XXXVIII.—DISTRIBUTION OF CASES OF CHOLERA IN VARIOUS EPIDEMICS AND SERIES OF EPIDEMICS (DATA FROM STICKER, 1912, p. 151).

The numbers of cases in each week of the epidemic were provided, and if there were an odd number of weeks, the cases of the middle week were divided equally between the first and second halves of the epidemic.

Epidemic	Prussia, 1848-59 (monthly)	Hamburg, 1831-73 (monthly)	Petrograd, 1831	Moscow, 1831	Berlin, 1831	Hamburg, 1831	Vienna, 1831	Prague, 1831	Lübeck, 1832	Rostock, 1832
Number of cases in the first period	193,146	12570·5	8370·5	5,204	1415·5	861	2,107	1,555	230·5	453·5
Number of cases in the second period	118,890	6823·5	434·5	1,047	772·5	77	1,380	1,831	906·5	226·5
Total ...	312,036	19394·0	8805·0	6,251	2188·0	938	3,487	3,386	1137·0	680·0
100 × number of cases in the first period divided by the total ...	61·9	64·8	95·1	83·3	64·7	91·8	60·4	45·9	20·3	66·7

epidemic was of the type described, some reflections suggest themselves. The explanation of the forms of epidemic curves is not definitely known (*see* Brownlee, Greenwood, Ross, Sticker), but the cause is only rarely the exhaustion of susceptible persons. It appears that from a certain point in time, often perhaps from the very commencement of the epidemic itself, the infectivity of the disease diminishes. If, therefore, we were to inject persons exposed to risk with coloured water, and

[1] It is to be remembered that, according to Eckert ("Die Rolle der Kontaktinfektion in der Epidemiologie der Cholera," *Berl. klin. Woch.*, 1913, 1, p. 2326), water-borne infection was not responsible for the outbreak of cholera among the Bulgarian troops before the Chataldja lines in the first Balkan War, an outbreak which cost them from 16,000 to 29,600 men and may quite possibly have saved Constantinople. Personal infection was thought to have played the chief part.

were so to plan our operations that the absolute number of persons inoculated in each unit of time, say each day or each week, was constant, and were finally to count up the numbers of inoculated persons and of attacks amongst them and prepare similar statistics of uninoculated persons, we should inevitably find that the attack-rate was lower in the former case. This would happen were the infectivity constant, and a declining infectivity would accentuate it.

We can illustrate the point readily. Let us suppose that an initial population of 1,000 is exposed to risk during a period of three weeks, that during each of these weeks the chance of acquiring a disease is one in a hundred and that at the beginning of each week fifty persons receive an injection of coloured water. At the end of the first week there will be 950 uninoculated and 50 inoculated persons, and ten cases of disease will have occurred—9·5 among the uninoculated, 0·5 among the inoculated. Stopping at this point, we have identical attack-rates in the two classes—viz., 1 per cent. At the beginning of the second week, we are left with 940·5 uninoculated and 49·5 inoculated men who have not had the disease; fifty of the former are now inoculated, so that the numbers become 890·5 uninoculated, 99·5 inoculated; subjecting all to the attack-rate of 1 per cent., 8·905 uninoculated and 0·995 inoculated fall victims. Adding up from the beginning we reach 100 inoculated with 1·495 cases and 900 uninoculated with 18·405 cases—rates of 15 and 20 per mille respectively. At the end of the third week we shall have 150 inoculated with 2·98 cases, and 850 uninoculated with 26·72 cases—rate per mille of 20 and 31; thus the relative advantage of the inoculated increases. This error, which is inherent in the method of summation adopted, would have been increased if the chance of infection instead of being constant had diminished as time passed —a phenomenon which we have seen reason to anticipate in practice. The conclusion is that, if we are only provided with the total number of inoculations performed during the epidemic and the allotment of attacks between inoculated and uninoculated classes, we are almost sure to find that the inoculated have an advantage and are by no means warranted in concluding that this is any more than a necessary consequence of the manner of compilation. Of course, the explanation of the fallacy is that the period of exposure to risk is not the same in the two classes; the men who would perhaps have been inoculated next week have a chance of acquiring the disease this week, and if they do so will naturally be counted as unvaccinated attacked persons. There is no way of circumventing

this error on the basis of summarizing tables and it must always, in greater or less degree, affect the statistics of inoculations performed during an epidemic. No doubt if full details as to time and place of inoculation and attack were furnished, we could isolate the true from the spurious advantage; but that, in the stress of active campaigning, such particulars are likely to be recorded is improbable. The conclusion seems to be that Savas's divisional statistics cannot be made the basis of sound reasoning with respect to the value of inoculation; it is possible, indeed, as we shall show directly, probable that inoculation did confer a benefit upon the troops, but we cannot express this numerically in a manner to be comparable with the typhoid results. A detailed inspection of Savas's figures only confirms the above conclusions. To begin with there are some arithmetical discrepancies. The table is copied accurately from Savas's paper, but the totals for once vaccinated and twice vaccinated persons are not correct—the sums of the columns are respectively 14,603 and 101,221, instead of 14,613 and 91,224. As in all but two instances the sums of the numbers once, twice, and not vaccinated agree with the numbers entered as the original strengths of the divisions, it is probable that for 22,199 twice vaccinated in the second division we should read 12,199: with this correction, the discrepancy is reduced to three in the case of the twice vaccinated and ten in the case of the once vaccinated, an unimportant difference. But there is some uncertainty about the true figures, for in the text the number of once vaccinated persons is given as 14,411.

Further inspections of the details reveal other peculiarities. In the case of the second division, every person once vaccinated and every person not vaccinated was attacked by cholera, and the same remark applies to the tenth division. In the eighth division every uninoculated man had cholera. Savas speaks so enthusiastically of the value of inoculation and lays so much stress on the experience of certain regiments that, had these divisional results really afforded a comparison as fair as it is striking, we do not doubt that he would have commented on them. The only conclusion we can draw is that all or the majority of the twice inoculated men in the two divisions cited became so after the uninoculated men were attacked, and this is perhaps what Savas means by saying that the second division was completely inoculated. For these reasons, we do not think the treatment adopted in the case of the other observers' material could deduce results of scientific value from Savas's divisional statistics.

This is disappointing because, among other things, if we could take the figures at their face value we could test certain statistical processes, since we have six instead of only four "cells" owing to the fact that there are records of twice, once, and not vaccinated men. We have, indeed, made a few calculations, but although they might have some interest from the theoretical side, the above-mentioned facts deprive them of practical value and we shall not reproduce the constants.

It will be seen, then, that the data of Table XXXVI do not of themselves add much to our knowledge of the value of inoculation, but combined with another set of observations (Table XXXVII) interesting results can be obtained. It will be remembered that one branch of the Greek Army—viz., the sanitary corps—was completely inoculated prior to the outbreak of cholera. The members of this corps were of the same social class as the combatants; they had indeed received a short course of sanitary instruction from the medical officers, but against this must be set the fact that they may have been more exposed to risk than ordinary soldiers; for instance, it was often their duty to carry cholera patients, sometimes on their backs and down mountain paths under conditions rendering it difficult or impossible to avoid contamination with fæces or vomit.

It seems reasonable to think that the sanitary service did not occupy a more but a less favourable position than the combatants as regards the risk of infection; the one particular in which they were distinguished from all or nearly all the combatants was, that practically every man had been twice vaccinated before the disease broke out. If now we construct a fourfold table (Table XXXIX), the divisions being

TABLE XXXIX.—SAVAS'S DATA.

	Not attacked	Attacked	Total
Sanitary corps	2,884	13	2,897
Combatants	112,613	2,192	114,805
Total	115,497	2,205	117,702

$\chi^2 = 32{\cdot}79$. P = less than 0·0001.

TABLE XL.—SAVAS'S DATA.

	Not attacked	Attacked	Total
Sanitary corps	2,884	13	2,897
Combatants	109,729	2,179	111,908
Total	112,613	2,192	114,805

$\chi^2 = 33{\cdot}85$. P = less than 0·0001.

between attacked and not attacked, as usual, and between combatants and sanitary corps, we find $\chi^2 = 32{\cdot}79$, so that the odds against the differences between the attack-rates being a mere chance event are very heavy. We must infer that the sanitary corps somehow or other acquired a higher degree of immunity than the combatants.[1] *If* we adopt Savas's view as to class of men and degree of exposure to risk, the only inference left is that the immunity was consequent upon early and complete inoculation. We have been at some pains to detect any source of material fallacy in this reasoning and have failed to do so. Assuming that it is correct, anti-cholera inoculation carried out with the technique likely to be used in the case of our own troops is a prophylactic step of importance, although an exact statistical measure of the degree of relative immunity conferred cannot be provided.

That anti-vaccinists will accept our conclusions respecting Savas's data is wildly improbable. We know no more about the Greek Sanitary Corps than Savas tells us. They may all have been vegetarians, or non-smokers, or red-headed, and all or any of these things may render them less likely to contract cholera, but we do not see why objections which no sensible man would allow to influence him in the affairs of ordinary life should suddenly acquire scientific importance when the question is one of interpreting statistics. Our conclusions, then, respecting the Greek experience are that, although no inference can be drawn from a comparison of the attack-rates upon inoculated and uninoculated soldiers in the combatant units, yet the striking difference between the incidence upon the sanitary corps and that upon the rest of the army is evidence in favour of the efficacy of the process.

Briefly summarizing our study of anti-cholera inoculation statistics, it appears that in several cases the difference between the inoculated and uninoculated in respect of cholera incidence is greater than can be attributed to the operation of chance. Were it permissible to combine the results, then the combined improbability of random sampling accounting for the deviations from the systems deduced from a hypothesis of strict independence would be great indeed.[2] Naturally, these conclusions presuppose the fulfilment of the material conditions laid

[1] It not being clear whether Table XXXVI does or does not include the data of Table XXXVII, we have repeated the calculation shown in Table XXXIX, assuming that the sanitary corps were already included in Table XXXVI. The difference is not of importance (Table XL).

[2] This combination has not been much used in practice where a series of tables for which the values of χ^2 are known is in question; but see Pearson and Heron, p. 314.

down at the beginning, and in each set we have, to the best of our ability, indicated whether such was the case or not. The test of

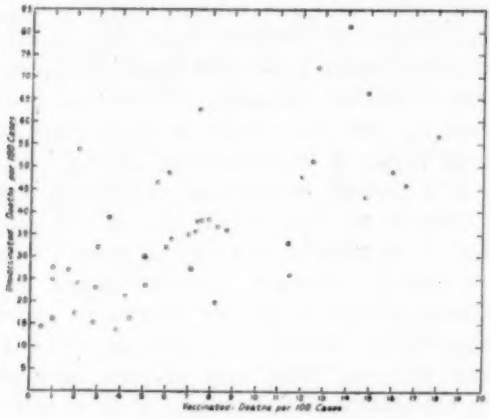


FIG. 1.

Correlation between small-pox case mortality for inoculated and uninoculated in various epidemics.

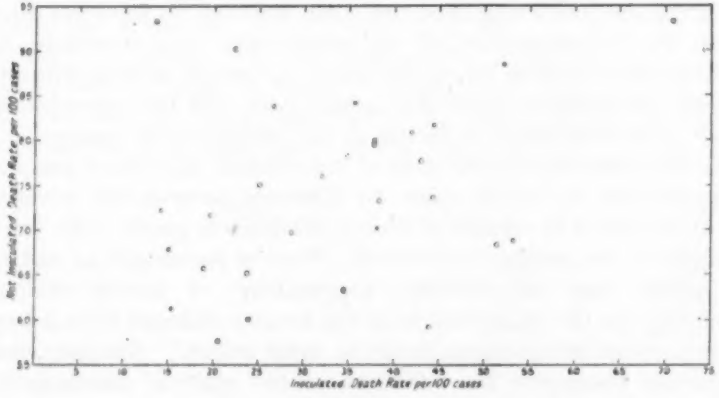


FIG. 2.

Correlation between plague case mortality for inoculated and uninoculated in various districts.

consistence to which we alluded (p. 116) cannot be satisfactorily applied to such short series and we ought to warn the reader that in the case

of the only two diseases for which we could obtain long series—i.e., many cases of groups of inoculated and uninoculated persons exposed to epidemics of different severity—the results are bad. This is illustrated in figs. 1 and 2. The first (Table XLI) refers to small-pox and

TABLE XLI.—SMALL-POX CASE MORTALITY. PERCENTAGE OF DEATHS AMONG THOSE ATTACKED ACCORDING TO WHETHER VACCINATED OR UNVACCINATED.

		Death-rate per 100 cases	
		Vaccinated	Unvaccinated
France	1816-41[1]	1·0	16·2
Marseilles	1828[1]	1·0	25·0
Würtemberg	1821-35[1]	7·1	27·3
,,	1840-50[1]	3·5	38·9
Canton Vaud	1825-9[1]	2·1	24·0
Bohemia	1835-55[1]	5·1	29·8
Milan	1830-51[1]	7·6	38·3
Verona	1828-39[1]	5·6	46.6
Breslau	1831-3[1]	2·1	53·8
Copenhagen	1828-37[1]	1·0	27·6
Vienna Hospital	1834[2]	12·5	51·2
,, ,,	1837-56[1]	5·0	30·0
,, ,,	1859[1]	3·8	13·8
,, ,,	1870[1]	2·0	17·4
Prague Children's Hospital	1840-58[1]	3·0	32·0
London Small-pox Hospital	1836-56[1]	7·0	35·0
,, ,, ,,	1863[1]	12·0	48·0
,, ,, ,,	1864[1]	8·7	36·0
,, ,, ,,	1865[1]	7·4	38·0
,, ,, ,,	1866[1]	7·3	35·7
,, ,, ,,	1867[1]	8·3	36·8
,, ,, ,,	1868[1]	6·2	34·0
,, ,, ,,	1870[1]	7·9	38·5
,, ,, ,,	1871[1]	14·9	66·5
Poland	1871[1]	11·4	33·0
Frankfort Town	1871[1]	16·0	49·0
,, ,,	1872[1]	16·6	46·0
,, Government 1 District	1871[1]	14·8	43·4
Liverpool Small-pox Hospital	1870[1]	12·7	72.0
Berlin Lazaretto	1871-2[1]	14·1	81·3
Coblentz	1871[1]	18·0	56·9
Quebec	1819-20[2]	1·7	27·0
Philadelphia	1825[2]	—	60·0
Darkehmen	1828-9[2]	—	18·8
Carniola	1834-5[2]	4·4	16·2
Carinthia	1834-5[2]	0·5	14·5
Adriatic	1835[2]	2·8	15·2
Lower Austria	1835[2]	11·5	25·8
Galicia	1836[2]	5·1	23·5
Dalmatia	1836[2]	8·2	19·6
Kiel	1852-3[2]	6·0	32 0
Malta	—— [2]	4·2	21·1
Epidemiological Society Returns	—— [2]	2·9	23·0
Illinois	—— [2]	6·1	48·6

[1] From Second Report of the Vaccination Committee, Appendix II, Table N (Dr. A. F. Hopkirk), from *Zeitschft. des K. Preuss. Stat. Bureau*, Jg. xiii, p. 155.

[2] From Sixth Report of the Vaccination Committee, Appendix X (Sir B. A. Whitelegge), p. 660. From article "Vaccination," by Dr. S. A. Abbott, in Wood's "Reference Handbook of the Medical Sciences." For references to originals *see* loc. cit.

the figures are taken from Appendix X of the Sixth Report of the Royal Commission on Vaccination and from Appendix II of the Second Report. The second (Table XLII) is compiled from returns of plague inocula-

TABLE XLII.—PLAGUE: CASE MORTALITY, AMONG INOCULATED AND UNINOCULATED PERSONS.[1]

		Inoculated			Uninoculated		
		Attacks	Deaths	Death-rate per 100 cases	Attacks	Deaths	Death-rate per 100 cases
Kirkee	1897	32	17	53·1	143	98	68·5
Dharwar ...	1898	129	54	41·9	1,100	889	80·8
Gadag	,,	193	83	43·0	278	216	77·7
Belgaum ...	,,	78	40	51·3	506	346	68·4
Ahmednagar ...	,,	70	31	44·3	563	415	73·7
Aden	1900	23	8	34·8	83	65	78·3
Nagpur	1906	25	8	32·0	204	156	76·0
Kollegal ...	1908	68	18	26·5	136	114	83·8
Kunagalli ...	,,	45	20	44·4	49	40	81·6
Mudigandam ...	,,	59	21	35·6	38	32	84·2
Madanhalli and Haranapuram	,,	135	51	37·8	234	186	79·5
Palayam ...	,,	73	28	38·4	60	44	73·3
Dharampur ...	1910	24	6	25·0	209	157	75·1
Gujranwala ...	,,	41	18	43·9	1,013	600	59·2
Amritsar District	,,	90	17	18·9	2,513	1,651	65·7
Neemuch ...	,,	39	8	20·5	382	220	57·6
Nagpur Mills ...	,,	74	10	13·5	7,770	7,253	93·3
Yeotmal ...	,,	27	6	22·2	407	367	90·2
Haribar... ...	,,	92	18	19·6	283	203	71·7
Palni	,,	89	63	70·8	179	167	93·3
Bijapur	1911	28	8	28·6	1,228	857	69·8
Gadag	,,	20	3	15·0	1,695	1,150	67·8
Salem	,,	434	226	52·1	1,693	1,495	88·3
Channapatna ...	,,	71	10	14·1	166	120	72·3
Kirkee	1912	21	8	38·1	158	111	70·3
Gadag	,,	39	6	15·4	599	367	61·3
Dayalpur ...	1903	29	10	34·5	60	38	63·3
Punjab	1901-02	881	209	23·7	266,700	173,732	65·1
,, ...	1902-03	3,399	814	23·9	49,433	29,723	60·1
Aden	1905	37	14	37·8	368	294	79·9

[1] Compiled from Reports of the Plague Research Laboratory, Bombay, with the exception of the last three entries, which are extracted from Sticker's "Die Pest," part ii, p. 449.

tions issued by the Government of India. It will be seen that there is very little tendency for the death-rates of vaccinated and unvaccinated persons to vary together, a result which should make us regard the data either with suspicion or as being untrustworthy owing to insufficiency of observations.

We have now completed that part of our inquiry which relates to

the determination of the probability that inoculation does really confer some advantage on the inoculated person and we have seen that such appears to be the case both for typhoid and for cholera. It is therefore natural to inquire how we can measure the degree of advantage, and how we can compare the advantage derived in connexion with one disease with that obtained in the case of another malady. The discussion of these points compels one to face numerous difficulties and we shall have to examine certain matters not at first sight germane to the subject of the paper. This will form the topic of the next section.

Section III: The Meaning of Immunization and the Interpretation of Consistent Data relating to Two or more Epidemics.

In the previous sections we discussed in the first place the conditions necessary for insuring that the comparison of death- or attack-rates amongst immunized and non-immunized should be legitimate, and the methods for judging whether (supposing such comparability assured) any observed difference might, or might not, be due to the mere chances of sampling. We then proceeded to examine the available data for two diseases of present importance, typhoid and cholera, and to show how often the necessary conditions of comparability were unfulfilled or the numbers of observations insufficient to ensure complete certainty of judgment as to the significance of the results. In Section I we pointed out that on *any* hypothesis as to the meaning of immunization it seemed obvious that the death- or attack-rates amongst immunized and non-immunized should be very highly correlated, assuming the numbers of observations sufficient to render the errors of sampling relatively small; but have just indicated that this *a posteriori* test of consistence completely breaks down for more than one set of data. Suppose, however, that we can find some data for which this test of consistence is fulfilled and for which accordingly, within the margin of errors of sampling, there is a single-valued functional relation between the death- or attack-rates amongst immunized and non-immunized. We now wish to discuss the problem whether any hypothesis as to the meaning of immunization can be framed of sufficient definiteness to enable us to predict the form of that functional relation.

Let us consider first the assumptions implied in the method adopted by Professor Pearson for the treatment of such "fourfold tables" as have been given above—a method which has been very

largely used by Macdonell, Maynard, and others. One or two such tables were given by Professor Pearson in his original memoir on the method (1900, ii) and a table for deaths amongst vaccinated and unvaccinated small-pox patients is headed in the following form:—

TABLE XLIII.

Degree of effective vaccination	Strength to resist small-pox when incurred		
	Deaths	Recoveries	Total
Cicatrix absent	94	383	477
Cicatrix present	42	1,562	1,604
Total	136	1,945	2,081

It is assumed, as suggested by the headings, that we have really to do with two continuous variables: (1) Degree of effective vaccination, or perhaps it would be better to say immunity; those with "cicatrix absent" representing the patients of lower immunity, and those with "cicatrix present" the patients of higher immunity. (2) Strength to resist small-pox when incurred: those who die being the patients of lower, and those who recover the patients of higher, strength. The two groups, in each case, it is also assumed, are contiguous, but not overlapping, being sundered from each other at some one definite value of the continuous variable hypothesized, just as one might sunder short men from tall by taking all men as short who were under 5 ft. 4 in., and all as tall who were over that limit. Further, it is assumed that the distribution of the variables follow the normal law of error. The distribution of "degree of immunity" may accordingly be represented by the annexed diagram, fig. 3 (*a*), in which the proportion of vaccinated has been taken at 77·08 per cent., the value in the above table, and the distribution of "strength to resist small-pox when incurred" might be represented by a similar diagram. Finally, supposing the correlation to be also of the normal form, the coefficient of correlation between "degree of effective vaccination" and "strength to resist small-pox when incurred" can be calculated. Later writers who have dealt with similar tables have not always inserted general headings indicating what they assumed the supposed variables to be, but Macdonnell (1902 and 1903), for instance, terms the coefficient he obtains the correlation between "effectiveness of vaccination and strength to resist the disease" or between "vaccination and strength of resistance."

We should ourselves prefer to head the columns of the above table with some such words as "Mildness of case," and to substitute for

"Degree of effective vaccination" "Resistance of patient when infected." Whatever assumption is made as to the form of the distribution, the effect of vaccination is to increase the resistance of the patient, but the mildness or severity of the case is dependent not only on the patient's resistance but also on the quantity and quality of the infection, and also possibly on external circumstances, so that the correlation between resistance and mildness of case is not unity and the resistance of the patient measures only the *chance* of his recovery. Looking at the matter from this point of view, we may ask ourselves what is, on Pearson's hypothesis, the law relating "chance of recovery" to

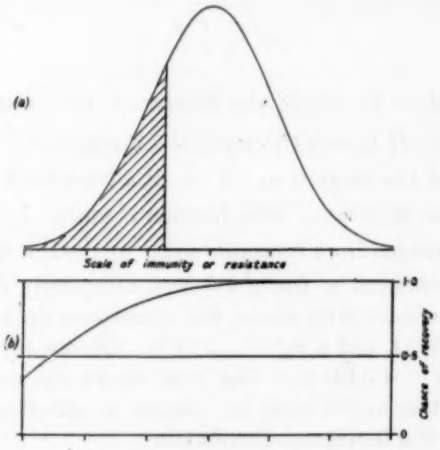


FIG. 3.

(*a*) Distribution of resistance on Pearson's theory; shaded area to the left being that of unvaccinated. (*b*) The chance of recovery for each degree of resistance.

"degree of effective vaccination," or, as we should prefer to term it, "resistance."

In fig. 4 let M_2M_2 be the vertical through the mean of the variable X_2, M the mean of the whole distribution, RR the line of regression through the means of rows, HH the vertical line of division, cutting off deaths to left and recoveries to right. Let r, σ_1, σ_2, be the correlation and standard deviations. Evidently as a horizontal array is moved downwards by, say, a unit the point at which HH cuts it moves to the left from the mean of the array by a constant amount,

that is, as the s.d. of the array is constant, by a constant fraction of the s.d. of the array. Hence the curve showing the chance of recovery for every value of the "resistance" X_1 must be a normal integral curve $C\,C$ as sketched on the right of the figure. To fix the curve we only require its centre (the point at which the chance is 0·5) and the distance from that centre of, say, the chances corresponding to normal deviations of unity—a distance which might well be termed the dispersion of the curve. The centre of the curve is given by the point P where the line $H\,H$ meets $R\,R$, for at that point $H\,H$ bisects the array, that is, the centre of the curve deviates from the mean of X_1 by an amount—

$$x_1 = \frac{1}{r}\,\frac{\sigma_1}{\sigma_2}\,h$$

where $h = H\,M_2$. To obtain the dispersion, note that at a distance s *from the point* P, $H\,H$ cuts the array at a distance $r\,\frac{\sigma_2}{\sigma_1}\,s$ from its mean; or, as the s.d. of the array is $\sigma_2\,\sqrt{1-r^2}$, at a fraction of the s.d. equal to $rs/\sigma_1\sqrt{1-r^2}$ from its mean. This fraction is unity—i.e., the array is cut by $H\,H$ at a distance from its mean equal to the s.d. when $s = \sigma_1\frac{\sqrt{1-r^2}}{r}$ The curve of chances is therefore now completely determined. For example, in Pearson's table above, the correlation on the assumption of normality is 0·5954, and $h/\sigma_2 = -1{\cdot}5114$. Hence $x_1 = -2{\cdot}5385\sigma_1$ and the dispersion $s = 1{\cdot}3495\ \sigma_1$. Fig. 3 (*b*) shows the curve in its proper position in relation to the scale of "degree of effective vaccination" or resistance, and the frequency distribution.

This form of law for the relation between resistance and chance of recovery looks to us a very reasonable one, but the assumption that the frequency distribution of resistance for vaccinated and unvaccinated together can be represented by a single normal curve as in fig. 3 (*a*) is quite another matter. The assumption implies that whatever the proportion of vaccinated (*a*) the resistance of no unvaccinated person is higher than that of a vaccinated person; (*b*) the resistance of the most resistant unvaccinated person is just equal to that of the least resistant vaccinated person[1]; (*c*) the frequencies of the most resistant unvaccinated persons and least resistant vaccinated

[1] Where the number of observations is large; where the observations are few, there may, of course, be appreciable differences between the resistances of adjacent individuals, ranked in order of resistance, but these differences decrease as the number of observations is increased and will be quite low except in the extreme tails of the curve.

persons are also equal. Surely an assumption with such implications is absurd on the face of it? Nor do matters look any better if we adhere to Professor Pearson's original wording: it is equally difficult to assent to an assumption which implies that no person without a cicatrix recorded is more "effectively vaccinated" than a person with cicatrix present; that the degree of effective vaccination of the most effectively vaccinated person with a cicatrix absent is always just equal to that of the least effectively vaccinated person with a cicatrix present; and that the frequency of the most effectively vaccinated persons with cicatrix absent is always just equal to the frequency of the least effectively vaccinated persons with cicatrix present. Possibly someone with greater imaginative powers than we

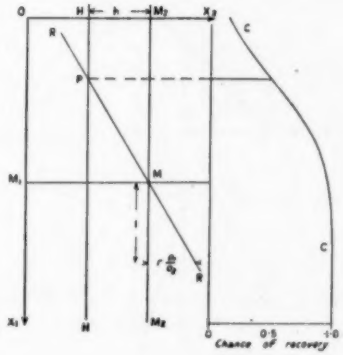


FIG. 4.

possess might be able to frame some hypothesis to illustrate how such a distribution might arise, even with the usual approximately random selection of persons for inoculation, but we must confess it has proved an impossible task to us. Matters become even more difficult when cases are considered in which varying proportions of the population have been inoculated under precisely the same conditions. Suppose, for example, that in one year one inoculates 25 per cent. of a certain population, choosing the persons for inoculation as near as may be at random: next year, with the same method and the same precautions one inoculates 75 per cent. What are the distributions of resistance, or "degree of effective vaccination"? Suppose the curve (*a*), fig. 5, to represent the distribution for the first year. How shall we place the curve for the second year? There seems no reason for supposing

that the bounding resistance between vaccinated and unvaccinated would be altered, so suppose we make these bounding values the same in the two years: curve (*b*) for the second year will then be placed as shown if we suppose the standard deviation also to be unaltered. But this makes the mean resistance of both vaccinated and unvaccinated much higher in the second year than in the first, in contradiction to our supposition that the whole process was carried out in precisely the

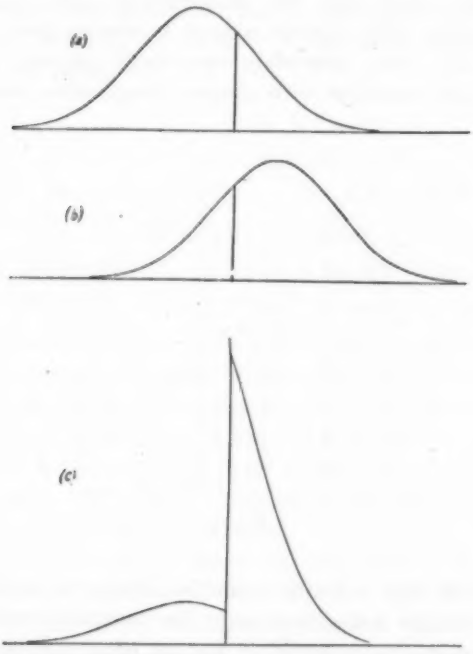


FIG. 5.

same way in both years, excepting only as regards the increase in the proportion of inoculated. If we decrease the standard deviation of the second curve, the agreement between the mean resistances in the two years becomes better for the inoculated and worse for the uninoculated: if we increase the standard deviation, the reverse is the case. If we give up the idea that the bounding resistance between inoculated and uninoculated is constant, we can make the mean resistance of the inoculated the same in the two years and also the mean resistance

of the uninoculated, *but by no shifting of the mean or alteration of the standard deviation can we make the distributions of the two groups similar in the first year and the second.* If in the first year the distribution of resistances is as given by fig. 5 (*a*) the distribution in the second year should, it seems to us, be given, not by a normal curve, but by the distribution (*c*) obtained from (*a*) by trebling all the ordinates for the vaccinated and taking one-third of each of the ordinates for the unvaccinated: the process being exactly the same in each year the form of the distribution of resistances for each group should be unaltered. We cannot see any way of avoiding this conclusion. The assumption that the distribution is of the form (*a*) in the one case is not consistent with its being of the same form in the second case.

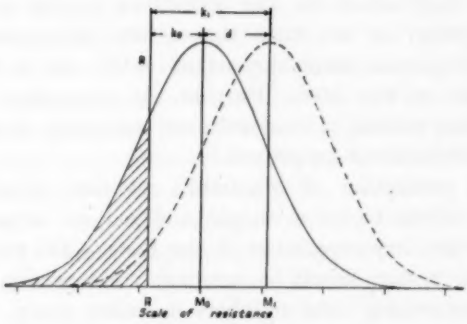


FIG. 6.

Distributions of resistance on Maynard's theory. Full line curve, unvaccinated; broken curve, vaccinated. M_0, M_1, means of unvaccinated and vaccinated. R, critical resistance, all of greater resistance recovering and of lower resistance dying.

The whole difficulty is, in fact, as one of us expressed it before (Yule, 1912, p. 638), that on this assumption one cannot effect a change in the numerical proportion of inoculated without changing them qualitatively at the same time.

This assumption as to the form of the distribution of "resistance" amongst the inoculated and uninoculated must in our judgment be rejected, and the question is whether some more reasonable assumption cannot be made. One has already been put forward by Maynard (1913, p. 67). "My conception of the problem," he writes, "is as follows: Members of a population possess the character 'power to

resist attack' in varying amounts, and the distribution of this population in regard to the character is continuous and probably capable of being represented, at least approximately, by a normal curve. Assuming inoculation to increase this 'power to resist attack,' its effect will be to bodily move the treated portion of this population in the direction of increasing amounts of this power, without, necessarily, altering the type of distribution." If, in fig. 6, the continuous normal curve represents the distribution of "resistance" amongst the uninoculated portion of the population in question, some curve more to the right, like the broken curve shown, will represent the distribution of resistance amongst that portion of the population that has been inoculated. We have shown the two curves as possessing the same standard deviation, as suggested by Maynard's wording, but this is not essential; the distribution for the inoculated population may possess a dispersion greater or less than that of the uninoculated group, the change in the dispersion being dependent on the way in which different individuals react to the dose. Further, the assumption that the two distributions are normal is evidently not essential, though necessary for simple mathematical treatment.

With this conception of Maynard's we find ourselves in entire agreement; it seems to us a simple and natural assumption which probably is a close representation of the facts. The two distributions are now shown, as they should be, as entirely independent; they may or may not largely overlap; the numbers in either group may be altered as largely as we please without altering the form of the distribution of resistance for that group.

A further assumption is also suggested by Maynard as to the portion of the population, whether inoculated or uninoculated, that is attacked or that dies as the case may be—the simplest assumption that can be made. Let us assume, that is to say, that all those are attacked or die who possess less than a certain degree of resistance, say the resistance R in fig. 6. Then the portions of the inoculated and uninoculated populations cut off to the left of the ordinate through R will be the portions attacked or dying, as the case may be. Some consideration suggests that this assumption is almost too simple to be a complete or true representation of what actually occurs, for it is difficult to suppose that in practice the chance of an individual being attacked depends only on his "resistance" and on no other circumstance. Let us take it, however, as a working hypothesis, develop its consequences and see how far it fits the facts.

As pointed out by Dr. Maynard, if the s.d.'s of the two curves are assumed to be the same, the results of a single epidemic suffice to determine the relative positions of the two curves. Thus, in the case of Professor Pearson's table which we have used as an illustration above, the proportion of recoveries amongst the unvaccinated ("cicatrix absent") is 0·8029. Reference to any table of the normal integral shows that this is equivalent to a normal deviation of 0·852 of the standard deviation. The mean M_0 of the distribution of resistance for the unvaccinated must therefore lie, taking the standard deviation as the unit of our scale, at a distance 0·852 of a unit to the right of the point we take as R. Similarly, the proportion of recoveries amongst the vaccinated is 0·9738, corresponding to a normal deviation 1·940, and M_1 the mean of the distribution for the vaccinated must lie at a distance 1·940 units to the right of R. The difference between the mean resistances of vaccinated and unvaccinated is $1{\cdot}940 - 0{\cdot}852$ or 1·088, the unit being the s.d. The diagram is drawn from these data, the numbers of vaccinated and unvaccinated being taken as equal.

But the assumption that the two standard deviations are equal seems to us unnecessary and undesirable. Suppose we take the s.d. of the distribution for the unvaccinated as unity, as before, the s.d. of the distribution for the vaccinated being σ_1, and the difference of the means $M_1 - M_0 = d$. A single epidemic no longer gives the necessary data for determining σ_1 and d. But suppose we have also data for a second epidemic, say of less intensity, cutting off all those whose resistance is less than S, then the distributions are determinate. For we have from fig. 6, noting that k_0 and k_1 are negative.

$$k_0 = k_1 + d \qquad (1)$$

The data give k_0 and the ratio k_1/σ_1, say a_1, so we may write this

$$k_0 = a_1\sigma_1 + d \qquad (2)$$

Inserting the values of k_0 and a_1 given by the two epidemics, we have a pair of linear equations for determining σ_1 and d.

But evidently, though we could determine in this way the two distributions of "resistance" consistent with the given data, no test would be afforded of the working hypothesis. To afford such a test we must have data for more than two epidemics and see how far they lead to the same constants for the distributions. The simplest way to carry out such a test is to work out the values of k_0 and a_1 for every epidemic in the series, plot them on a diagram and see whether, within the limits of fluctuations of sampling, the points obtained lie on a straight line.

The diagram will suffice to show whether there is an approximate fit, but for accurate work a straight line must be fitted to the observed points by some method that gives a precise answer, and further, we must be able to determine the probable true position of the more or less erroneous point in order to decide whether the differences between the observed and the true positions of the points are within the permissible limits. These problems are by no means easy to solve. If the probable errors of the two co-ordinates are the same, and if the distribution of the points may be taken as given by the normal law of error, the principal axis is the most probable true position of the line—it is the line which makes the sum of the squares of the perpendiculars from the observed points a minimum. But in fact the probable errors of the two co-ordinates are not the same, for the numbers of observations are never the same for inoculated and uninoculated: and, we may add, the probable errors are not the same for different points, as the number of observations varies from one epidemic to another. Further, there is little reason to suppose that the correlation is normal. In these circumstances it does not seem possible to give any thoroughly satisfactory general solution. Failing anything better, we decided to work with the principal axis; it gives a good, if not the theoretically best, solution—and a unique solution. It also may be shown that the true line must lie between the lines of regression, and this the principal axis does. We have not attempted any system of weighting the points to allow for the varying numbers of observations; this would have greatly increased the work, increased it indeed to a greater extent than the trustworthiness of the observations would seem to justify, and it did not seem worth while to attempt to develop such a method. The procedure we have used was therefore as follows :—

(1) Work out for each epidemic of the series the percentages of recoveries (or not attacked) amongst inoculated and uninoculated.

(2) Look up these proportions in a table of the normal integral and substitute the corresponding normal deviations, k_0 and a_1.

(3) Work out the means, standard deviations and correlation coefficient for the normal deviations.

If r, s_0, s_1, are the values found for the correlation and standard deviations, the angle θ which the principal axis makes with the axis of k_0 is, by a well-known formula, given by—

$$\tan 2\theta = \frac{2r\,s_0 s_1}{s_0^2 - s_1^2} \qquad (3)$$

The axis is then determined completely by the fact that it passes through the mean of the observations. To illustrate the work we have given the arithmetic for our first illustration in full.

But this is only the first part of the problem. Having obtained the line, or what we hope may be regarded as a fair approximation to its true position, what is the probable position on that line of the true point corresponding to a given observed point? The solution we have adopted is this: Suppose a perpendicular to be dropped from the observed point on to the principal axis; the foot of this perpendicular is approximately the true position of the point. Again, this solution is not strictly correct, but we judge it to be sufficiently so for our purpose. Here, as in obtaining the approximate position of the true line, we have endeavoured to minimize errors by selecting only cases in which the correlation is high. Supposing the equation obtained between k_0 and a_1—i.e., the equation to the principal axis—to be

$$k_0 = a_1\sigma_1 + d$$

the "corrected" value k_0' of any observed value k_0, that is the value which we judge to be approximately the true value, is given by

$$k_0' = \frac{a_1\sigma_1 + d + \sigma_1^2 k_0}{1 + \sigma_1^2} \qquad (4)$$

a_1 being the observed value associated with the observed k_0. The corrected value a_1' is most simply obtained by substituting k_0' in the equation to the line.

Illustrations.

After the remarks made at the end of the preceding section, it is not wonderful that we found considerable difficulty in discovering data which would afford any really satisfactory test of the theory. So inadequate in fact are the data, that we regard the cases given in the following pages as examples rather than as tests. For an adequate test we feel that recourse must be had to experiment, and on a somewhat large scale; failing such experimental data, we have in the meantime endeavoured to supplement our examples by considering cases in which the *differentia* between the two classes is no longer inoculation. It is evident, in fact, that the two curves of fig. 6 need not represent the distributions of resistance amongst inoculated and uninoculated respectively. The one might represent the distribution

of resistance, say, for males and the other for females: or the one for whites and the other for negroes. The theory, if applicable at all, should be applicable to many cases besides the comparison of inoculated and uninoculated individuals, indeed to cases in which we are not even concerned with data respecting sickness or mortality.

(*A*) *Cholera Inoculation.*—This was one of the first cases that we tried. In order that we might not have to deal with large errors of sampling and in the hope of thereby obtaining a more consistent series, we decided to exclude all cases in which χ^2 was less than 10. This would have left us with only three of Haffkine's cases, however, and this seemed rather few to deal with. But Nijland appears to have used the same method as Haffkine, so it seemed legitimate, as far as method of inoculation was concerned, to regard his material as on a par with that of Haffkine, though it differs in as far as his data refer in part to natives of the Dutch colonies and in part to Europeans. The Europeans we did not think it right to include with natives, and this left us with only three of Nijland's cases also. The six cases so sorted out afford the following data:—

TABLE XLIV.—CHOLERA INOCULATION: HAFFKINE'S AND NIJLAND'S DATA.

Place	Proportion not attacked		Normal deviations	
	Not inoculated	Inoculated	Not inoculated k_0	Inoculated a_1
(1) Calcutta ...	0·8776	0·9892	−1·1631	−2·2974
(2) Margherita ...	0·7687	0·9796	−0·7346	−2·0456
(3) Cachar ...	0·9698	0·9953	−1·8779	−2·5972
(4) Singaradja ...	0·9901	0·9988	−2·3301	−3·0370
(5) Semarang ...	0·9935	0·9990	−2·4838	−3·0902
(6) Selong ...	0·9941	0·9990	−2·5181	−3·0902

The two columns on the right show the normal deviations equivalent to the proportions of "not attacked" in the first two columns, and the diagram of fig. 7 shows how closely the points they give are grouped round a straight line.

These normal deviations give the following means, standard deviations, and coefficient of correlation, the subscript 0 referring to the not inoculated:—

$$M_0 = -1{\cdot}85127 \qquad M_1 = -2{\cdot}69293$$
$$s_0 = 0{\cdot}68255 \qquad s_1 = 0{\cdot}41205$$
$$r = 0{\cdot}99154$$

Hence from equation (3)—

$$\tan 2\theta = +1{\cdot}8836165$$
$$2\theta = 62°\,2'{\cdot}8$$
$$\theta = 31°\,1'{\cdot}4$$
$$\tan\theta = 0{\cdot}60141 \qquad \cot\theta = 1{\cdot}66275$$

As tan θ is the slope of the line to the axis of k_0, cot θ gives the ratio of a deviation in k_0 to a deviation in a_1, or the equation to the principal axis may be written—

$$k_0 + 1{\cdot}85127 = 1{\cdot}66275\ (a_1 + 2{\cdot}69293)$$

or—

$$k_0 = 1{\cdot}66275\ a_1 + 2{\cdot}62640 \qquad (5a)$$

This is the form in which we require the equation to compare with (2) on p. 149. It may also be convenient to have it for calculation in the form—

$$a_1 = 0{\cdot}60141\ k_0 - 1{\cdot}57956 \qquad (5b)$$

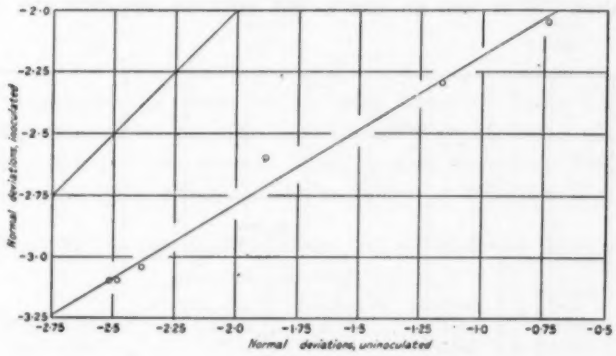


FIG. 7.

Cholera. Correlation between normal deviations for inoculated and uninoculated. The diagonal line in the upper left-hand corner is the line that would have been obtained if inoculation had been completely ineffective.

The line in fig. 7 is plotted from these equations. Equation (5a), comparing it with (2), tells us that, taking the standard deviation of the distribution of resistances for the uninoculated as unity, the standard deviation of the distribution for the inoculated is 1·66, and the mean of the same is 2·63 units higher than the mean resistance of the uninoculated. Fig. 8 shows the two curves drawn from these data, assuming the same number in each class, and the verticals numbered 1, 2, 3, 4, 5, 6 (determined by the method described below) correspond as closely as possible to the values of k_0 and a_1 in the last two columns of Table XLIV. In the Calcutta epidemic, that is to say, the proportions of inoculated and of uninoculated attacked are given very closely by the fractions of the two normal curves cut off to the left of the ordinate

through 1, all those with a resistance less than 1 being attacked. In the Margherita epidemic the proportions are similarly given by the fractions cut off to the left of the ordinate 2, and so on. While the mean resistance of the inoculated is very much higher than that of the uninoculated, their resistance is also much more variable; 94·3 per cent. of the inoculated have a higher resistance than the mean of the uninoculated, while only 4·3 per cent. of the uninoculated have a higher resistance than the mean of the inoculated.

We have stated above that the verticals, 1, 2, 3, 4, 5, 6, correspond "as closely as possible" to the observed values of k_0 and a_1 in Table XLIV. It is evident that they cannot, in general, correspond exactly, for equation (5) is not an exact, but only an approximate, relation

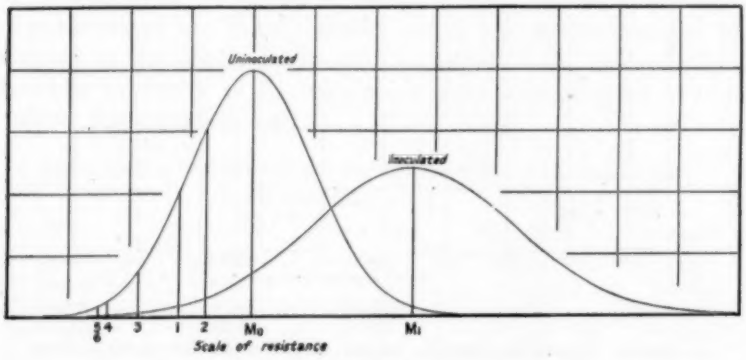


FIG. 8.

Cholera. Distributions of resistance of uninoculated (to the left) and inoculated (to the right). The side of one of the squares is the standard deviation for the uninoculated, and the two curves have equal areas—viz., ten squares. 1, 2, 3, &c., are the critical resistances for the six epidemics.

between k_0 and a_1, the closeness of fit being shown by the line in fig. 7. If, then, we plotted the verticals from the values of k_0, the values of a_1 would not be correct; if we plotted them from the values of a_1, the values of k_0 would not be correct, and positions intermediate between these two must be taken as probably the true positions. We estimated the true positions as described above, p. 151. Thus the corrected value of k_0 is given by

$$k_0' = \frac{1{\cdot}66275\, a_1 + 2{\cdot}76474\, k_0 + 2{\cdot}62640}{3{\cdot}76474} \qquad (6)$$

and for the Calcutta epidemic we have $k_0' = -1{\cdot}1712$ against $-1{\cdot}1631$ observed. The corrected value of a_1 is given by inserting the value of k_0' in the equation

$$a_1 = 0{\cdot}60141\, k_0 - 1{\cdot}57956 \qquad (7)$$

and we thus find $a_1' = -2{\cdot}2839$ (against $-2{\cdot}2974$ observed). Proceeding in this way we find the following corrected values—

TABLE XLV.

	k_0'	a_1'
(1)	−1·1712	−2·2839
(2)	−0·7453	−2·0278
(3)	−1·8285	−2·6792
(4)	−2·3549	−2·9958
(5)	−2·4912	−3·0778
(6)	−2·5164	−3·0929

A comparison of these values with those in Table XLIV shows that the corrections are not, in most cases, great, but a simpler comparison results, if we convert these normal deviations back into the corresponding proportions, as in the following table:—

TABLE XLVI.—CHOLERA INOCULATION: FIT OF THEORY AND OBSERVATION.

	Proportions not attacked			
	Observed	Calculated	Difference	Standard error	Ratio Diff./s.e.
(1) Not inoculated	0·8776	0·8790	−0·0014	0·01412	0·10
Inoculated	0·9892	0·9888	+0·0004	0·00619	0·06
(2) Not inoculated	0·7687	0·7720	−0·0033	0·03478	0·09
Inoculated	0·9796	0·9787	+0·0009	0·01010	0·09
(3) Not inoculated	0·9698	0·9662	+0·0036	0·00211	1·71
Inoculated	0·9953	0·9963	−0·0010	0·00090	1·11
(4) Not inoculated	0·9901	0·9907	−0·0006	0·00029	2·05
Inoculated	0·9988	0·9986	+0·0002	0·00054	0·37
(5) Not inoculated	0·9935	0·9936	−0·0001	0·00028	0·36
Inoculated	0·9990	0·9990	—	0·00035	0·00
(6) Not inoculated	0·9941	0·9941	—	0·00020	0·00
Inoculated	0·9990	0·9990	—	0·00045	0.00

The observed proportions are repeated in the first column; in the second are given the values corresponding to the "corrected" normal deviations above, and in the third column the differences between the corrected and observed values. These differences are then compared with their standard error. It will be seen that only a single difference exceeds twice the standard error, the twelve being grouped as follows: Less than the standard error, nine; exceeding the standard error, but not twice the standard error, two; exceeding twice the standard error, one. If the distribution of errors were normal the expectation would

be 8·2 : 3·3 : 0·5. The fit is thus a very fair one, but much stress must not be laid on this in view of the fact that our cases are stringently selected.

Fig. 9 shows the curve relating the proportions of not attacked amongst inoculated and uninoculated, corresponding to the straight line of fig. 7 relating the normal deviations: the diagonal across the figure is the form the curve would take if the proportions of not attacked amongst inoculated and uninoculated were equal. It may be remarked that, over the range covered by observation, the divergence from linearity of the relation between the two proportions is not conspicuous. Fig. 10 shows the form taken by the complete curve, from an intense

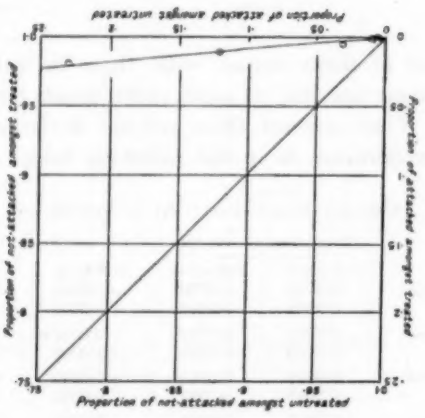

Proportion of not-attacked amongst untreated
Proportion of not-attacked amongst treated
Proportion of attacked amongst treated
Proportion of not-attacked amongst untreated

FIG. 9.

Cholera. Relation between proportions of not-attacked amongst treated and untreated; curve calculated from the two normal distributions. To read off the proportions of attacked reverse the diagram.

epidemic in which everyone is attacked, down to a very mild attack in which practically no one is attacked: observation only covers the last quarter of this curve. In an inset, we have drawn the extreme end of the curve, in the right-hand upper corner, to a greatly enlarged scale, in order to bring out a point regarding its form. In fig. 9 and in the general diagram of fig. 10 the curve appears to be tangential to the horizontal at its upper end, but it is really tangential to the vertical, as it crosses the diagonal at a value of p_1 equal to 0·999963 approximately and then turns upwards as shown in the inset. The reason for this is

obvious on considering the diagram of the two normal curves (fig. 8). The curve for the inoculated has a larger standard deviation than the other, and must therefore ultimately cut it towards the left and rise above it. We have, then, only to carry the vertical like 1, 2, 3, &c., sufficiently to the left to make the proportion of not attacked among the uninoculated greater than among the inoculated. The point at which this will occur is given by the two equations (5) (*a*) and (*b*) (p. 153) for this case, which give us $k_0 = a_1$ when

$$k_0 = 0{\cdot}60141\, k_0 - 1{\cdot}57956 = -3{\cdot}9628 \qquad (8)$$

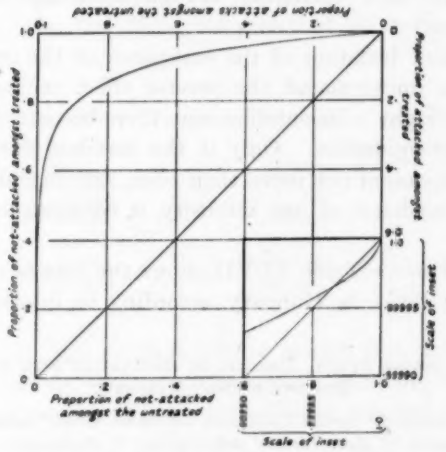

Proportion of not-attacked amongst the uninoculated
Proportion of not-attacked amongst treated
Proportion of not-attacked amongst the untreated
Proportion of attacked amongst treated
Scale of inset

FIG. 10.

The complete curve of fig. 9. The right-hand end of the curve appears tangential to the horizontal. As a fact it is tangential to the vertical. The inset represents the right-hand extremity of the curve on a scale enlarged 4,000 diameters. When the proportion of not attacked amongst the untreated exceeds 0·999963 the proportion of not attacked is lower amongst the untreated.

that is, when the proportion of not attacked amongst the uninoculated is 0·999963 as stated above. It is extremely doubtful whether we could trust the normal law at deviations greatly exceeding three times the standard deviation, so that the point is, perhaps, of more theoretical than practical interest. But it is worth noting that distributions may exist in which the crossing point comes much lower in the scale of proportion not attacked. Thus, suppose the means of the two normal distributions of fig. 8 differed by only a single unit instead of by

2·626 units, then the proportions of recoveries would be equal when k_0 was 1·5088, or the proportion of not attacked 0·9343. When the proportion of not attacked among the uninoculated was greater than this, the proportion of not attacked among the inoculated would be less than the proportion among the uninoculated. It is an interesting question whether such distributions ever arise in practice; their existence would imply that the process of inoculation actually weakened resistance in some cases, so that, although the mean resistance was raised, the proportion of the population with less than a certain small resistance was increased and inoculation consequently became disadvantageous for mild epidemics, though advantageous for more or less virulent ones.

If the standard deviation of the resistance of the inoculated is less than that of the uninoculated, the reverse effect occurs, as the curves cut towards the right. Inoculation may then become disadvantageous for very virulent epidemics. Only if the standard deviations are the same the curves cannot cut more than once, and inoculation, if advantageous for an epidemic of one intensity, is advantageous for all other intensities.

(*B*) *Swine Fever.*—Table XLVII shows the results of serum treatment for swine fever in Hungary, according to information obtained

TABLE XLVII.—SWINE FEVER. RESULTS OF INOCULATION WITH SERUM ALONE OF HEALTHY SWINE. HUNGARY.

(From a pamphlet entitled "Serum Treatment for Swine Fever," issued by the National Federation of Meat Traders' Associations, in September, 1913.)

Number of herds	Treated	Died		Untreated	Died	
		Head	Per cent.		Head	Per cent.
15	1,419	—	—	899	97	10·9
15	2,183	54	2·4	1,572	201	12·7
3	360	29	7·9	219	57	26·0
6	1,108	179	16·1	1,029	461	44·8
3	190	40	21·0	254	120	47·2
4	229	70	30·4	1,035	615	59·4
46	5,489	372	8·8	5,008	1,551	30·9
6	599	383	63·8	659	382	57·9
52	6,088	755	12·4	5,667	1,933	34·1

through the British Consulate. It is not quite clear on the face of it how the table is compiled. We assume that it is to be read as follows. There were fifteen test herds in which none of the treated

swine died; of the untreated swine in these herds, of which there were 899, 97, or 10·9 per cent., died. There were also fifteen herds in which some, but (probably, we think) not more than 5 per cent. of the treated swine died, 54 out of 2,183 in these herds dying, or 2·4 per cent.; of the untreated swine in these herds, numbering 1,572, 201, or 12·7 per cent., died—and so on. But we do not understand why the group of six herds in which 63·8 per cent. of the treated swine died is tabulated separately at the foot of the table, nor why the results for this group are so divergent. Omitting the first line of the table, inasmuch as a percentage zero, which is presumably due only to the chances of sampling, corresponds to a normal deviation of infinity, and also omitting the last divergent group of six herds, we have the following data :—

TABLE XLVIII.—SWINE FEVER. DATA USED AND DERIVED FROM TABLE XLVII.

Proportions of survivals			Normal deviations		
Untreated	Treated		Untreated (k_0)		Treated (a_1)
0·8721	0·9753		−1·1364		−1·9651
0·7397	0·9194		−0·6424		−1·4011
0·5520	0·8384		−0·1307		−0·9879
0·5276	0·7895		−0·0692		−0·8047
0·4058	0·6943		+0·2384		−0·5081

The diagram of fig. 11 shows how closely the points given by these normal deviations lie to a straight line. Working out the means, standard deviations and coefficient of correlation, we find :—

$$M_0 = -0{\cdot}34806 \qquad M_1 = -1{\cdot}13338$$
$$s_0 = 0{\cdot}48520 \qquad s_1 = 0{\cdot}50667$$
$$r = 0{\cdot}99610$$

whence

$$\tan 2\theta = -22{\cdot}99343$$
$$2\theta = 92^\circ\ 29'{\cdot}4$$
$$\theta = 46^\circ\ 14'{\cdot}7$$
$$\tan\theta = 1{\cdot}0444 \qquad \cot\theta = 0{\cdot}9575$$

The equation to the principal axis may therefore be written in either of the forms—

$$\left.\begin{aligned} k_0 &= 0{\cdot}9575\, a_1 + 0{\cdot}7371 \quad \text{(a)} \\ a_1 &= 1{\cdot}0444\, k_0 - 0{\cdot}7699 \quad \text{(b)} \end{aligned}\right\} \qquad (9)$$

The line of fig. 11 is drawn from these equations and fig. 12 shows the two distributions of resistance, for equal numbers of inoculated

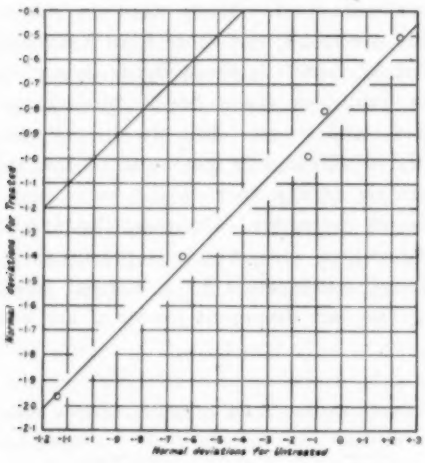


FIG. 11.

Swine fever. Relation between normal deviations corresponding to proportions of recoveries amongst treated and untreated animals respectively.

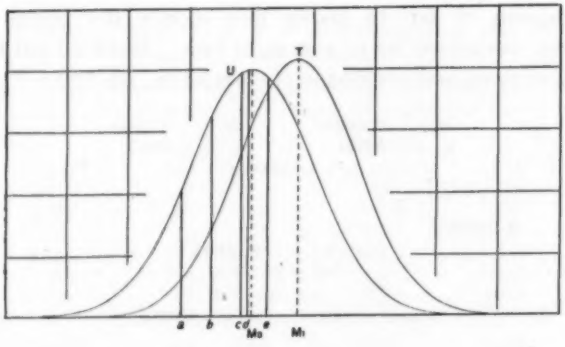


FIG. 12.

Swine fever. Distributions of resistance of untreated animals (to the left) and treated (to the right). The areas of the two curves are equal; if each represents 1,000 animals, one square is 100. The unit of the horizontal scale is the standard deviation of the untreated. *a*, *b*, *c*, *d*, *e*, are the critical resistances for the five groups of herds affected by outbreaks of varying severity.

and uninoculated. Taking the standard deviation for the uninoculated as unity as before, the standard deviation for the inoculated is 0·9575, and the mean of the inoculated is 0·7371 unit higher than the mean of the uninoculated. We are uncertain whether the swine fever data are fatality-rates in the strict sense or deaths in terms of a population some members of which were never attacked; the cholera data are, of course, incidence-rates. But the conclusions as regards the distributions of resistance are very different. In the case of cholera the means differed by more than 2·5 units, here they differ by less than 0·75. For cholera, the dispersion of the resistance of the inoculated was much greater than that of the uninoculated. For swine fever the dispersion of the resistance of the inoculated animals is slightly the lower of the two. A glance at the two diagrams shows the nature of the difference more effectively than any verbal description.

Evaluating the "corrected" normal deviations as in the last illustration, we find—

TABLE XLIX.

		k_0'		a'_1
(1)	...	−1·1406	...	−1·9611
(2)	...	−0·6226	...	−1·4201
(3)	...	−0·1714	...	−0·9489
(4)	...	−0·0505	...	−0·8226
(5)	...	+0·2448	...	−0·5142

and hence we have the following comparison of the observed proportions of survivals with the calculated proportions:—

TABLE L.—SWINE FEVER.—FIT OF THEORY AND OBSERVATION.

		Proportions of recoveries		Difference	Standard error	Ratio Diff./s.e.
		Observed	Calculated			
(1)	Untreated	0·8721	0·8730	−0·0009	0·00842	0·11
	Treated	0·9753	0·9751	+0·0002	0·00332	0·06
(2)	Untreated	0·7397	0·7332	+0·0065	0·0297	0·22
	Treated	0·9194	0·9222	−0·0028	0·0144	0·19
(3)	Untreated	0·5520	0·5680	−0·0160	0·0155	1·03
	Treated	0·8384	0·8287	+0·0097	0·0111	0·87
(4)	Untreated	0·5276	0·5201	+0·0075	0·0313	0·24
	Treated	0·7895	0·7946	−0·0051	0·0296	0·17
(5)	Untreated	0·4058	0·4033	+0·0025	0·0153	0·16
	Treated	0·6943	0·6964	−0·0021	0·0304	0·07

The fit in this case is even closer than in the last, as the difference in only one case exceeds the standard error. Fig. 13 shows graphically the fit of the theoretical curve to the observations, and the form of

the entire theoretical curve. At the lower left-hand corner the curve actually sweeps round below the diagonal and terminates tangentially to the horizontal, this being an example of the theoretical case in which inoculation may become disadvantageous for very virulent epidemics. But the point at which the diagonal is cut by the curve lies even farther beyond the sphere of practical politics than in the last illustration. From equations (9) we find that $k_0 = a_1$ when

$$k_0 = 1{\cdot}0444\, k_0 - 0{\cdot}7699 = 17{\cdot}339$$

and this value of k_0, if the normal law continued to hold good for such extreme deviations, would correspond very nearly to a proportion

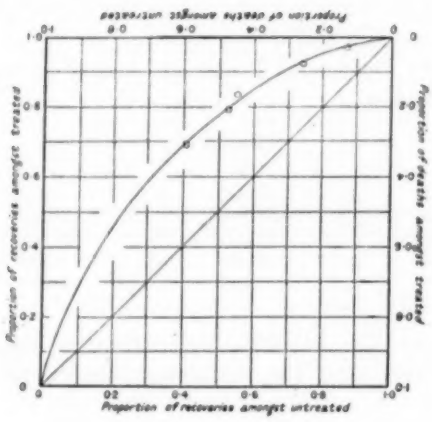

Proportion of recoveries amongst treated
Proportion of recoveries amongst untreated
Proportion of deaths amongst treated
Proportion of deaths amongst untreated

FIG. 13.

Swine fever. Relation between proportions of recoveries or deaths in the case of treated and untreated animals. The observed points are given by small circles; the curve is calculated from the two frequency distributions of resistances given in fig. 12.

of survivals represented by a unit in the sixty-sixth place of decimals.

The two preceding illustrations may seem very inadequate as examples of the theory with which we are dealing, but they are the only illustrations based on statistics of inoculation that we can give, for the reasons already mentioned. We turned, therefore, to data from other sources, but even here found it difficult to discover thoroughly satisfactory material. Unless the number of observations

is large, fluctuations of sampling are of considerable importance, the correlation between the death-rates or the equivalent normal deviations in the two groups is only moderate, and it becomes doubtful how far the principal axis represents the true position of the line we are endeavouring to determine; it is also difficult to find material in which the death-rates exhibit any very great range, and unless the range is considerable a very poor test is afforded of the linearity of the regression between the normal deviations.

The first illustration that follows is on the whole the best of several similar cases that we tried.

(*C*) *Diphtheria Fatality at Two Age-groups.* — From the Annual Reports of the County Medical Officer for London we extracted the figures for notified cases and deaths from diphtheria at the age-groups 0-5 and 5-10, in each of the years 1892 to 1913, and worked out the fatality-rates for each sex. Calculating the equivalent normal deviations, we then took each sex separately and, regarding the age-group 0-5 as the standard, or as if it were the group of the uninoculated, proceeded as before to find the distributions of resistance. The results were as follows, the subscript 0 referring to the lower age-groups:—

Males.

M_0 −0·77566 M_1 −1·31126
s_0 0·37420 s_1 0·26252
r 0·98010
tan θ 0·69673 cot θ 1·43528
$k_0 = 1{\cdot}43528\ a_1 + 1{\cdot}10637$
$a_1 = 0{\cdot}69673\ k_0 - 0{\cdot}77084$ (10)

Females.

M_0 −0·75261 M_1 −1·28105
s_0 0·36156 s_1 0·29197
r 0·97849
tan θ 0·80384 cot θ 1·24402
$k_0 = 1{\cdot}24402\ a_1 + 0{\cdot}84104$
$a_1 = 0{\cdot}80384\ k_0 - 0{\cdot}67607$ (11)

Figs. 14 and 15 show the distribution of the points about the principal axes for the respective cases, and figs. 16 and 17 the distributions of resistance. It will be noticed that the correlations are lower in these cases than in the last two illustrations, but the numbers of observations are not large, ranging from about 1,000 to 2,700. We have not worked out the complete comparison of the proportions derived from the corrected normal deviations with the actual proportions,

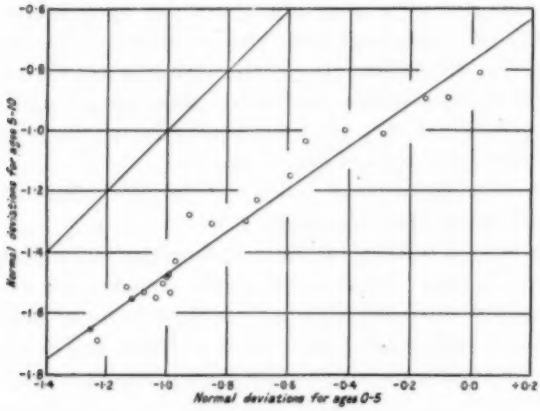


FIG. 14.

Diphtheria, males. Correlation between normal deviations at ages 0-5 and 5-10.

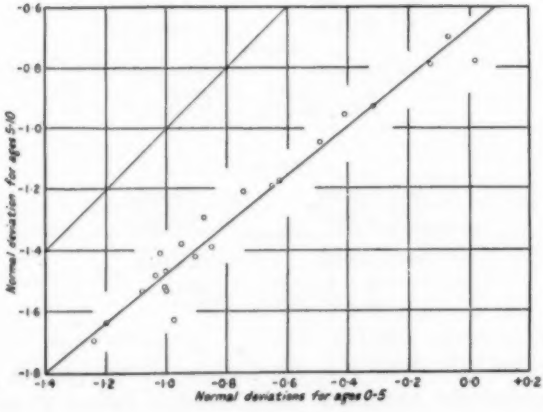


FIG. 15.

Diphtheria, females. Correlation between normal deviations at ages 0-5 and 5-10.

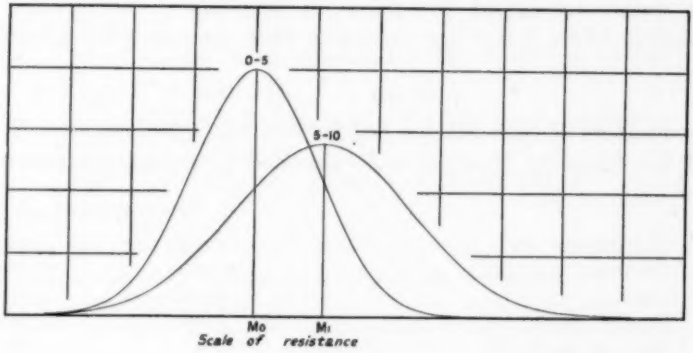


FIG. 16.

Diphtheria, males. Curves of resistance at ages 0-5 and 5-10. The area of each curve is ten squares, and the side of a square is the s.d. of the distribution for 0-5.

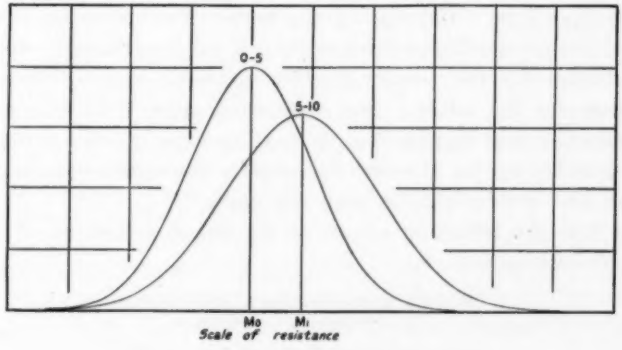


FIG. 17.

Diphtheria, females. Curves of resistance at ages 0-5 and 5-10. The area of each curve is ten squares, and the side of a square is the s.d. of the distribution for 0-5.

and of the differences with their probable errors, as in illustrations *A* and *B*, but the following test on one of the points does not suggest that deviations exceed the limits of sampling. One of the most divergent points is that for females in 1905, for which the co-ordinates are

$$k_0 = -0{\cdot}97421 \qquad a_1 = -1{\cdot}62841$$

The numbers of observations being 1,285 and 1,160 respectively. We find the following observed and calculated proportions of recoveries.

	Proportion of recoveries	
	0-5	5-10
Observed	0·8350	0·9483
Calculated	0.8547	0·9364
Difference	0·0197	0·0119
Standard error	0·0104	0·0065
Ratio diff./standard error	1·89	1·83

In each case the difference is less than twice the standard error.

So far we have treated each sex by itself, but it was natural to attempt to carry the work rather further and to endeavour to obtain all four distributions on a common scale. Females 0-5 showed slightly the highest mean fatality, judging from the mean normal deviation, so it was decided to take them as the standard. But it is clear that a difficulty may arise. Correlating the normal deviations for males 5-10 with the normal deviations for females 0-5, we determine the distribution of resistances for the former directly in terms of the distribution of resistances for the latter. But correlating males 5-10 with males 0-5, and males 0-5 with females 0-5, we have another indirect determination of the distribution for the first in terms of the distribution for the last, and the two determinations may not agree.

We find the following equations for the distributions of males in terms of females 0-5 :—

Males 0-5 against Females 0-5 ($r = 0{\cdot}99062$)

$$k_0 = 0{\cdot}96591\ a_1 - 0{\cdot}00339 \qquad (12)$$

Males 5-10 against Females 0-5 ($r = 0{\cdot}99597$)

$$k_0 = 1{\cdot}37900\ a_1 + 1{\cdot}05562 \qquad (13)$$

From equations (10) and (12) we have

$$\text{s.d. Males 5-10}/\text{s.d. Males 0-5} = 1{\cdot}43528$$
$$\text{s.d. Males 0-5}/\text{s.d. Females 0-5} = 0{\cdot}96591$$

Therefore

$$\text{s.d. Males 5-10}/\text{s.d. Females 0-5} = 1{\cdot}43528 \times 0{\cdot}96591 = 1{\cdot}38635$$

as compared with the directly determined value from equation (13) 1·37900.

The agreement seems to us very satisfactory and the mean of the two results, 1·383, may be taken as close to the truth. As regards the means :—

By (10)—

Mean Males 5-10 − mean Males 0-5 = 1·10637 × s.d. Males 0-5

by (12)—

Mean Males 0-5 − mean Females 0-5 = −0·00339 × s.d. Females 0-5

also by (12)—

s.d. Males 0-5 = 0·96591 × s.d. Females 0-5.

Therefore, finally,

Mean Males 5-10 − mean Females 0-5 = 1·06526 × s.d. Females 0-5

as compared with the directly determined value 1·05562.

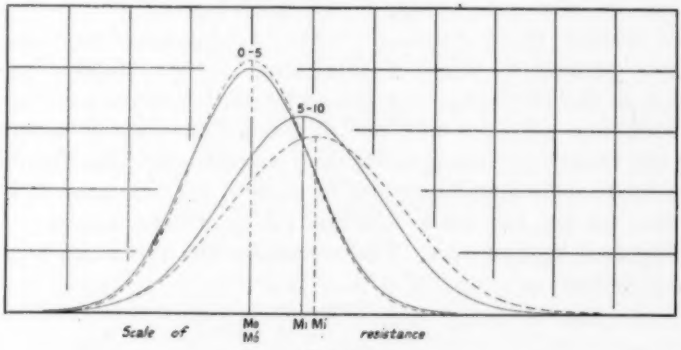


FIG. 18.

Diphtheria, males and females. Curves of resistance of the two sexes at ages 0-5 and 5-10 on the same scale. Full line curves, females; broken curves, males. The area of each curve is ten squares, and the side of a square is the s.d. of the distribution for females 0-5.

Again the agreement seems very fair, and we may take the average value, 1·060, as representing approximately the truth.

The distributions of resistance for the four groups therefore stand approximately as follows :—

	Mean	Standard deviation
Males 0-5	−0·003 ...	0·966
Females 0-5, taken as	0 ...	1·000
Females 5-10	0·841 ...	1·244
Males 5-10	1·060 ...	1·383

The distributions are shown in fig. 18. The mean percentages of recoveries for the four groups in the order given—that is to say, the

percentages corresponding to the mean normal deviations—are 0·781, 0·774, 0·900, 0·905, so that, as stated above, females aged 0-5 have a lower average proportion of recoveries than males of the same age, *but they show a higher (or sensibly the same) mean resistance.* The advantage obtained by males, within the range of fatalities in our data, is due to the lower variability of their resistance. For $k_0 = -0{\cdot}1$ almost exactly, or a proportion of recoveries 0·5398, males and females show the same fatality: for lower proportions of recoveries amongst females that sex has the advantage, if our assumption holds good. The most striking feature of the four distributions is the rise in the standard deviation *pari passu* with the rise in the mean resistance.

The next example we do not regard as a very satisfactory one, as the correlations are relatively low. We only give the results in brief, as they are interesting to compare with the foregoing.

(*D*) *Scarlet Fever Fatality at Two Age-groups.*—The data were precisely parallel to those of illustration *C*—viz., fatality-rates for London at the two age-groups 0-5 and 5-10, but at the moment when the work was done we only had at hand data for the years 1892-1910 and these were consequently the years utilized. The distributions of resistance were first determined for each of the two sexes separately, and then for the two sexes together, taking females aged 0-5 as the standard as in the last case. The results for the two sexes separately were as follows:—

Males.

$M_0 = -1{\cdot}4827$ $\quad M_1 = -2{\cdot}0757$

$s_0 = 0{\cdot}1403$ $\quad s_1 = 0{\cdot}1148$

r 0·8332

$$\left.\begin{aligned} k_0 &= 1{\cdot}2715\, a_1 + 1{\cdot}1567 \\ a_1 &= 0{\cdot}7865\, k_0 - 0{\cdot}9097 \end{aligned}\right\} \qquad (14)$$

Females.

$M_0 = -1{\cdot}5327$ $\quad M_1 = -2{\cdot}1228$

$s_0 = 0{\cdot}1503$ $\quad s_1 = 0{\cdot}1084$

r 0 8193

$$\left.\begin{aligned} k_0 &= 1{\cdot}4847\, a_1 + 1{\cdot}6190 \\ a_1 &= 0{\cdot}6735\, k_0 - 1{\cdot}0905 \end{aligned}\right\} \qquad (15)$$

In this case again we only tested the magnitude of the divergences of the points from the line in a single case. The point that looked to us one of the most divergent was that given by the observations for males in 1900. For this point the co-ordinates are

$k_0 = -1{\cdot}5040$ $\quad a_1 = -2{\cdot}2801$

and the numbers of observations are 2,052 and 2,300 respectively. We find the following observed and calculated proportions of recoveries:—

	Proportion of recoveries	
	0-5	5-10
Observed	0·9337 ...	0·9887
Calculated	0·9447 ...	0·9848
Difference	0·0110 ...	0·0039
Standard error	0·00549 ...	0·00220
Ratio diff./s.e.	2·00 ...	1·77

In the first case the difference is just over twice the standard error, in the second case less than twice. It does not seem, therefore, that on the whole the divergences exceed the limits of errors of sampling.

From the additional calculations necessary for reducing all distributions to females aged 0-5 as standard we find—

Males 0-5 *against Females* 0-5.

r 0·9726.

$$\left.\begin{aligned} k_0 &= 1{\cdot}0736\, a_1 + 0{\cdot}0591 \\ a_1 &= 0{\cdot}9314\, k_0 - 0{\cdot}0551 \end{aligned}\right\} \quad (16)$$

Males 5-10 *against Females* 0-5.

r 0·8211.

$$\left.\begin{aligned} k_0 &= 1{\cdot}3861\, a_1 + 1{\cdot}3444 \\ a_1 &= 0{\cdot}7215\, k_0 - 0{\cdot}9699 \end{aligned}\right\} \quad (17)$$

The indirect calculation gives—

s.d. Males 5-10 / s.d. Females 0-5 = 1·3651.

against the directly calculated value 1·3861, mean 1·3756. The indirect calculation gives the difference of the means as 1·3009 times the s.d. for females 0-5 against the directly determined value 1·3444 (a poor agreement this time), mean 1·3226. The distributions of resistance for the four groups (fig. 19) therefore stand approximately as follows :—

	Mean	Standard deviation
Females 0-5, taken as	0 ...	1·000
Males 0-5	0·059 ...	1·074
Males 5-10	1·323 ...	1·376
Females 5-10	1·619 ...	1·435

We note the same characteristic as in the last illustration of increasing standard deviation accompanying the rising mean. Further, while the mean normal deviation for the females aged 0-5 corresponds to a slightly higher proportion of recoveries than the mean normal deviation of the males of the same age—viz., 0·9373 against 0·9309—the mean resistance of the former is lower than that of the latter. This is just the reverse of what we noticed in the case of the diphtheria fatalities, where a *lower* proportion of recoveries amongst the females was accompanied by a *higher* resistance. In the present case again the

females exhibit the highest and lowest mean resistances, while in the case of diphtheria the males took the extreme positions.

Let us add that neither in this case nor the last do we desire to put forward such conclusions as can be drawn from the forms found for the respective distributions of resistance, as anything more than interesting illustrations of the points that may be suggested by the use of the method on adequate data. The data used, in our opinion, are not adequate to enable anything but the most tentative conclusions to be drawn, and we have not yet discussed several difficulties connected with the method.

(*E*) *Infant Mortality for the Two Sexes.*—It occurred to us that this might form an interesting case as, by taking districts of very varying

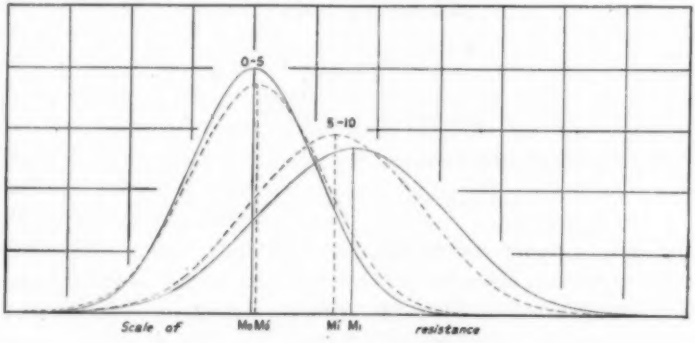


FIG. 19.

Scarlet fever, males and females. Distributions of resistance of the two sexes at ages 0-5 and 5-10 on the same scale. Full line curves, females; broken curves, males. The area of each curve is ten squares, and the side of a square is the s.d. of the distribution for females 0-5.

character, or possibly different countries, data for a considerable range of mortality could be obtained. Ultimately we used two sets of data :—

(I) We first turned to the English registration data given in the decennial supplements for 1881-90 and 1891-1900. As we wanted large districts, so as to keep fluctuations of sampling small, we first of all picked out certain urban registration districts, and then, in order to extend the range of mortality, added some counties of a more or less rural character. The first set of districts was thus made up as follows : (*a*) thirty London Registration Districts, 1881-90 and 1891-1900; thirty Lancashire Registration Districts, 1881-90 and 1891-1900; thirteen

Rural Registration Counties, 1881-90 and 1891-1900; total for the two decades, 146. (*b*) We then added, in order further to extend the material in the direction of low mortality, forty Administrative Counties in 1912—namely, those counties in which the number of births for each sex exceeded 1,500—making the total number of observations used, 186. (*c*) Finally, in order a little further to extend the line in the direction of high mortality, we added data for thirteen Bavarian districts in 1911, making the total number of observations used 199.

(II) The second set of data was still more heterogeneous. In the two volumes published under the title of "Statistique Internationale" by the Statistique de la France and edited by M. March, data are given for most of the principal countries of the world showing infantile mortality for each sex separately by quinquennial periods, from the earliest epoch at which the statistics could be obtained (cf. the first volume, pp. 463-64, and the second volume, p. 124). Plotting the mortality for the females against that for the males, it seemed that a few countries gave rather divergent results, showing a female mortality that was relatively high compared with the male mortality.[1] For this reason the figures for Sweden before 1800, Italy, Bulgaria, Servia, and Roumania were omitted. The data included, which, so far as one could judge from the rough graphic test, seemed fairly homogeneous, covered the following numbers of quinquennial periods in the countries stated:—

England and Wales	12
Scotland	11
Ireland	9
Denmark	12
Norway	14
Sweden	22
Finland	9
Russia	3
Austria	11
Hungary	3
Switzerland	8
Prussia	7
Bavaria	14
Saxony	11
Wurtemberg	8
Baden	8
Holland	12
Belgium	13
France	21
New South Wales	2
New Zealand	2
Total	212

[1] A few obvious errors in the rates were corrected by reference to the original data, also given in the volume.

These sets of data gave the following results :—

I (*a*) The subscript 0 refers to the male sex.

$$\begin{array}{ll} M_0 \ -0{\cdot}98029 & M_1 \ -1{\cdot}10294 \\ s_0 \ \ 0{\cdot}13509 & s_1 \ \ 0{\cdot}13629 \end{array}$$

$$r \ 0{\cdot}98108$$

$$\left.\begin{array}{l} k_0 = 0{\cdot}9910\, a_1 + 0{\cdot}1127 \\ a_1 = 1{\cdot}0091\, k_0 - 0{\cdot}1138 \end{array}\right\} \qquad (18)$$

I (*b*)

$$\begin{array}{ll} M_0 \ -1{\cdot}05515 & M_1 \ -1{\cdot}17975 \\ s_0 \ \ 0{\cdot}19261 & s_1 \ \ 0{\cdot}19538 \end{array}$$

$$r \ 0{\cdot}98610$$

$$\left.\begin{array}{l} k_0 = 0{\cdot}9858\, a_1 + 0{\cdot}1079 \\ a_1 = 1{\cdot}0144\, k_0 - 0{\cdot}1094 \end{array}\right\} \qquad (19)$$

I (*c*)

$$\begin{array}{ll} M_0 \ -1{\cdot}03512 & M_1 \ -1{\cdot}16008 \\ s_0 \ \ 0{\cdot}20490 & s_1 \ \ 0{\cdot}20652 \end{array}$$

$$r \ 0{\cdot}98675$$

$$\left.\begin{array}{l} k_0 = 0{\cdot}9920\, a_1 + 0{\cdot}1157 \\ a_1 = 1{\cdot}0080\, k_0 - 0{\cdot}1167 \end{array}\right\} \qquad (20)$$

II

$$\begin{array}{ll} M_0 \ -0{\cdot}91042 & M_1 \ -1{\cdot}02427 \\ s_0 \ \ 0{\cdot}23977 & s_1 \ \ 0{\cdot}23086 \end{array}$$

$$r \ 0{\cdot}99880$$

$$\left.\begin{array}{l} k_0 = 1{\cdot}0386\, a_1 + 0{\cdot}1534 \\ a_1 = 0{\cdot}9628\, k_0 - 0{\cdot}1477 \end{array}\right\} \qquad (21)$$

In the case of the original 146 districts of set I (*a*) the three worst points were given by the Strand District in 1891-1900, the City of London in the same decade, and Garstang in 1881-90. Using the same method as before, we find the following values for the observed and calculated death-rates and the ratio of the differences to their standard errors.

District	Observed	Calculated	Diff./s.e.
Strand	0·280	0·267	1·41
	0·215	0·229	1·62
City	0·252	0·243	1·09
	0·197	0·207	1·29
Garstang	0·113	0·119	0·76
	0·091	0·084	1·04

These figures do not suggest that in set I (*a*) at all events the deviations observed are greater than may be expected from fluctuations of sampling alone. Fig. 20 illustrates the correlation for the complete set I (*c*), and fig. 21 the correlation for set II.

It will be noticed that the results of the first series are fairly self-consistent, the additions to set I (*a*) of the forty additional districts included in I (*b*) and the thirteen additional districts included in I (*c*) not making any great alteration in the values obtained for the standard deviation and mean of the female distribution of resistance, which centre round 0·99 and 0·11 respectively. This set therefore shows the females to be slightly more resistant than the males and also to have a slightly lower variability, by which the advantage is increased.

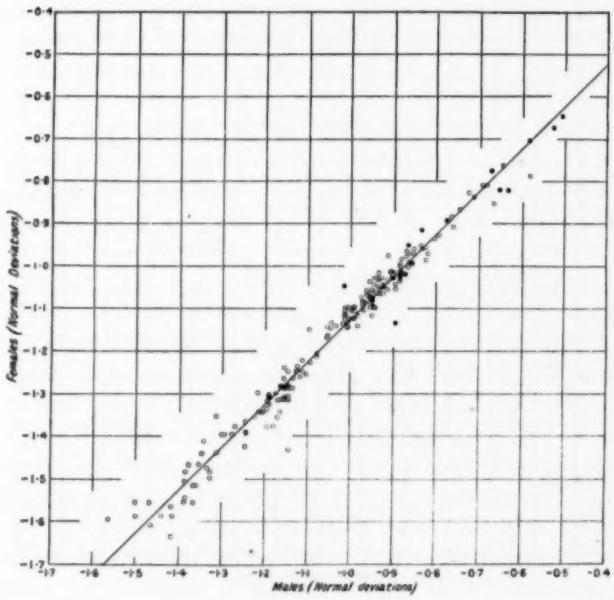


FIG. 20.

Correlation between normal deviations of infantile mortality, male and female. Circles, English districts; blocked-in circles, Bavarian districts.

But the second set of data gives a rather different result. This shows the mean resistance of the females as relatively higher, 0·15 units greater than the male mean instead of 0·11, but their standard deviation is higher than that of the males instead of lower, being 1·04 units approximately, not 0·99. It might appear that for possible moderate rates of mortality this greater variability might more than counterbalance the greater mean resistance of the females; but this

is not so. By equations (21) the distributions shown in set II would only give equal mortalities for males and females when

$$k_0 = 0{\cdot}9628\, k_0 - 0{\cdot}1477 = -3{\cdot}970$$

and this value of k_0 corresponds to the impossibly low mortality of 36 per million.

The correlation given by the second set of data is the higher of

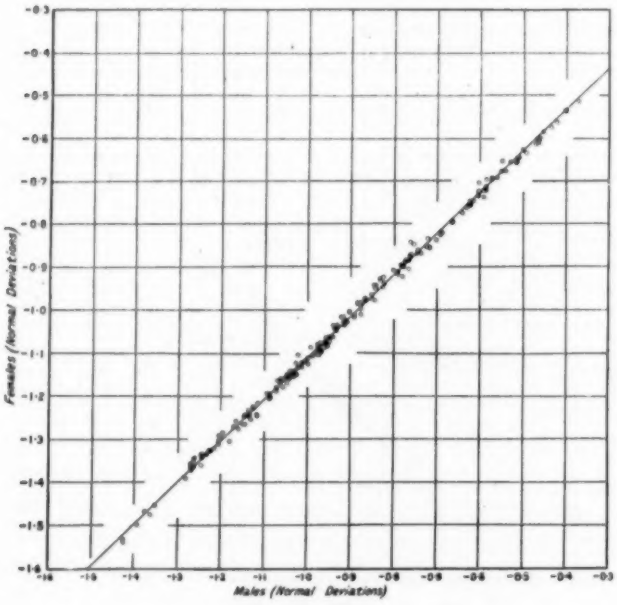


FIG. 21.

Correlation between normal deviations of infantile mortality, male and female, in various countries. Data from the *Statistique Internationale*.

the two, and the result given by that set is the less likely to be considerably affected by fluctuations of sampling. The question therefore arises whether the s.d. of the resistance of the females is really and generally higher than that of the males as given by the second set, the fact that the first set gives a lower value being perhaps only due to a fluctuation of sampling. Unfortunately we cannot give a definite answer to this question as, so far as we know, the standard error

of the slope of the principal axis (tan θ) has not yet been evaluated by anyone else and we have not evaluated it ourselves. Probably it is of the same order as the slope of the regression lines, but it may, we think, be higher. If we take it as of the same order, we may regard it as given approximately by $\sqrt{1-r^2}/\sqrt{n}$ (the geometric mean of the standard errors of the two regressions). This comes to 0·0115 for set I (*c*) and only 0·00336 for set II, or 0·0120 for the difference between the two. The actual difference is 1·0386 – 0·9920 or 0·0466, which is 3·9 times the standard error so estimated. On the whole it seems, therefore, unless we have considerably underestimated the standard error, that the difference is greater than is likely to be due to errors of sampling alone.

When we had reached in our work the stage to which we have at present carried the reader, we must confess that we began to feel somewhat uncomfortable concerning the accuracy with which the theory appeared to hold good; it held good more closely than we felt it had any right to.

In the first place, it did not seem to us very likely *a priori* that the distributions of resistance would be normal distributions. It seemed more probable, as there must be something like an absolute limit towards low or zero resistance, that both distributions would be skew, with longer tails towards high than towards low resistance and that the distribution with the higher mean would be less skew than the other. Why then, we asked ourselves, did the regression generally seem to be very closely linear? To investigate this question, we thought it would be interesting to work out the actual curves that would result if we assumed the two distributions of resistance to be skew distributions of known form. If we took this form to be that of one of Professor Pearson's well-known skew frequency curves, the calculation of the area up to each point on the scale would necessitate a good deal of work, so to make a first rough test we assumed the number of observations within successive intervals of the scale to be given by a binomial series. The following were then taken as pairs of distributions of resistance:—

I (*a*) The binomial $p = 0{\cdot}2$, $q = 0{\cdot}8$, $n = 20$. (*b*) The same binomial placed one interval higher on the scale.

Both distributions in this case were therefore very skew and equally skew.

II (*a*) The same binomial as the above. (*b*) The binomial $p = 0{\cdot}3$,

$q = 0{\cdot}7$, $n = 20$. These binomials had the same origin, so that the mean of the first is 4, and of the second 6.

III (*a*) The same binomial as in (*a*) above. (*b*) A normal curve, placing the mean of the curve at the upper limit of the interval on the binomial which includes the frequency of six successes, and making the interval 0·5 of the s.d. of the normal curve. These distributions give the following correspondent normal deviations:—

Binomial $(0{\cdot}2 + 0{\cdot}8)^{20}$	The same one place higher	Binomial $(0{\cdot}3 + 0{\cdot}7)^{20}$	Normal curve
−2·273	—	−3·156	−3·0
−1·483	−2·273	−2·428	−2·5
−0·820	−1·483	−1·807	−2·0
−0·224	−0·820	−1·243	−1·5
+0·331	−0·224	−0·715	−1·0
+0·857	+0·331	−0·211	−0·5
+1·361	+0·857	+0·274	0
+1·849	+1·361	+0·746	+0·5
+2·326	+1·849	+1·209	+1·0
+2·794	+2·326	+1·665	+1·5
+3·239	+2·794	+2·115	+2·0

The diagram (fig. 22) shows the lines plotted from the three pairs of distributions mentioned. It was plotted by us, of course, to a considerably larger scale and we confess that the result surprised us. The curvature of the line is in each case very slight—extremely slight in Case II, which represents most closely the forms we thought such distributions likely to assume—and only becomes evident owing to the considerable range covered. But the range covered by the data in most of our illustrations is very small, amounting to a little more than a unit of the scale, as will be seen from the following summary:—

Illustration	Normal deviations of untreated or standard		Range
	Lowest	Highest	
Cholera	−2·52	−1·16	1·36
Swine fever	−1·14	+0·24	1·38
Diphtheria (males)	−1·26	+0·02	1·28
,, (females)	−1·24	+0·02	1·26
Scarlet fever (males)	−1·70	−1·27	0·43
,, ,, (females)	−1·81	−1·27	0·54
Infantile mortality (II)	−1·42	−0·37	1·05

Considering the smallness of the range it is not surprising that curvature of the degree shown in the figure does not make itself evident, and it is clear that we must not regard the apparent linearity of the

regression over the very small range that is in general available as proving that the assumption of normality is justified. Given a pair of skew distributions of resistance with certain means and standard deviations, there may well be a pair of " equivalent " normal distributions which will give, over a wider range than will generally be available in practice, so nearly the same relation between normal deviations that the two will hardly be distinguishable within the limits of sampling; but these normal distributions will not have the same means and standard deviations as the original distributions. Thus the line for the two binomials of Case II is fitted very closely over the whole range by the

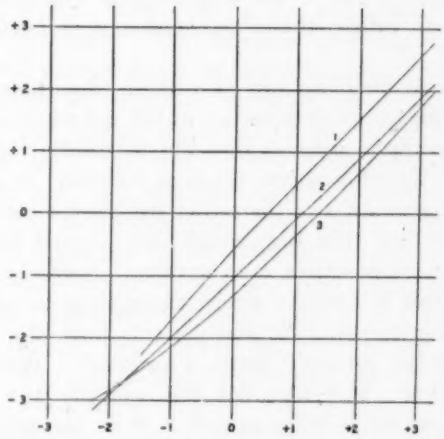


FIG. 22.

Relation between normal deviations, assuming distributions of resistance to be (1) the binomial $(0{\cdot}2 + 0{\cdot}8)^{20}$, and the same moved one place up; (2) the same and the binomial $(0{\cdot}3 + 0{\cdot}7)^{20}$; (3) the binomial of the first case and a normal curve.

straight line, of which the constants were determined by our usual method,

$$x = 1{\cdot}0456\, y + 1{\cdot}0609$$

That is, the ratio of the standard deviation of the second distribution to that of the first is given as 1·0456 and the difference of the means as 1·0609 times the s.d. of the first. But, actually, the standard deviations are 1·78885 and 2·04939, ratio 1·14565: while the means

are 4 and 6, difference 2, and 2/1·78885 is 1·11804. The binomial distributions have, therefore, more widely different standard deviations and means than the two normal curves which are, from our present standpoint, approximately equivalent.

The test applied is, of course, a rough one, and we quite admit that the subject deserves much more investigation than we have at present been able to give to it, but we think it may be definitely concluded that such illustrations as have been given above must not be held to prove, can hardly even be said to create any strong presumption, that the distributions of resistance, assuming the hypothesis otherwise true, are of the normal form given by the calculations. The solution of the problem as to the forms of the distributions which is given by our method is formal rather than true, and our difficulty falls to the ground.

But our second difficulty was of a more important character, and was brought strongly into prominence by the last numerical illustrations given. The hypothesis assumes that the mortality (supposing that we are dealing with the alternatives death or recovery) is strictly selective in the most stringent sense of the term: the disease cuts down all those with a resistance less than the critical value R, and permits all those with a resistance higher than R to survive. Surely, even in the case of fatality-rates this is rather a strong assumption to make? It seems at first sight easy to say: All the patients have the same disease—what determines the patient's death or survival? Why, of course, the patient's resistance. But may not the quantity and quality of the infection enter into the decision as well? If so, the patient's resistance determines, not the definite fact of death or recovery, but only the *chance* of death. The case against so simple a hypothesis as that on which we have been working is the same if we are dealing not with fatality-rates, but with attack-rates. Even if the population has been limited to that in which all the individuals are more or less at risk, are they all equally exposed to infection in the same degree? If not, again, the *chance* of infection may be a single-valued function of the resistance (of course, a resistance different from the resistance to death), but infection cannot be determined by the unique fact of the resistance being greater or less than some critical value R. In our last illustrations we were dealing not even with the fact of infection by a single definite disease, but with the general death-rate from all sorts of diseases. How can it be said in such a case that death or survival is uniquely determined by the quality of the patient? It must be admitted

that the element of chance or circumstance enters largely. How then does it come about that the hypothesis fits the facts as closely as it does?

The solution, we believe, may be found by considering how, on our present hypothesis, Professor Pearson's theory of the fourfold table must be transformed. Referring to Table XLIII on p. 142 and fig. 4, and following Professor Pearson with a slight change of nomenclature, we regarded OX_1 on the figure as the scale of "resistance," and OX_2 as the scale of "mildness of attack." The vertical HH cuts off deaths to left and recoveries to the right, and a horizontal line, not inserted on the figure, would, on Professor Pearson's theory, cut off vaccinated below and unvaccinated above. Now, consider the same table from the standpoint of the present method. We have been regarding the distributions of vaccinated and unvaccinated as distinct normal distributions and have usually spoken of them as distributions of *resistance*. But we may equally well regard them, not as distributions of resistance on the axis. OX_1 of fig. 4, but as the distributions of "mildness of attack" on the axis OX_2, *mildness of attack being only correlated with resistance*, and the distributions being shifted to the left, across HH, without altering their forms or their relative positions, if the intensity of the epidemic is increased. The correlation surface for "resistance" and "mildness of attack" will now be a compound normal surface, though two simplifying assumptions may well, we think, be made. We may assume, that is to say, (1) that the line of regression RR is continuous through both surfaces, otherwise there would be a discontinuity in the relation between resistance and mean mildness of attack, and (2) that the standard deviation of the array is the same for both surfaces, otherwise there would be a discontinuity in the law relating chance of recovery to resistance. On these assumptions, the law relating chance of recovery to resistance is the same as on Pearson's hypothesis—viz., the curve of chances is a normal integral curve.

We suggest then that we have determined, by the method used in our illustration, the two curves corresponding to "mildness of attack" in this case of small-pox fatality, and not the two curves of resistance. In many cases it may be difficult to give a definite interpretation or a precise name to these two curves; we should be hard put to it, for example, to assign a term to the scale if we were dealing with a fourfold table for vaccination and attack, or for sex and infantile mortality. But the point is this: It may be either (*a*) that the epidemic

or other cause of death (or disease) simply cuts down all those with a resistance lower than a certain critical value, or (*b*) that chance enters largely into the matter, and we can only say that the chance of death is a function of the resistance; if this function be of a certain form (the normal integral function if the correlation be normal) then (*a*) is necessarily a possible geometrical interpretation of the facts even though (*b*) is the true interpretation. For the given form of function relating chance of recovery to resistance, which seems a reasonable sort of form, we cannot distinguish between the two hypotheses (*a*) and (*b*)—so long, of course, as we cannot obtain some independent measure of resistance. The process may not be so crude, nor so strictly selective as it appears on our diagrams, like fig. 8 or fig. 12, showing a couple of normal distributions of "resistance" and a series of verticals marking off the quick from the dead in epidemics of increasing intensity. The relative forms of the two distributions of resistance may in truth be more or less different, and the division between the quick and the dead not sharp but blurred; given not by a vertical which moves across the two distributions, if the intensity of the epidemic is raised, and separates the survivors at increasing values of the resistance, but given by a curve of chances of the form of the normal integral curve which is moved to the right without altering its form, so that every chance of death p, which formerly corresponded to a resistance x, now corresponds to, say, a resistance $x + d$.

The reader will naturally ask if we cannot go further than this and determine in some way the true distributions and the form of the curve of chances. The answer, so far as we can see, must be in the negative. Certainly the fourfold table tells one absolutely nothing. The table (XLIII) on p. 142, for example, merely informs us that there are 1,604 observations in the correlation surface between resistance and mildness of attack for the vaccinated, and 477 in the corresponding surface for the unvaccinated. The vertical HH of fig. 4 cuts off forty-two observations to the left in the first surface and ninety-four in the second. All this tells us nothing about the correlation in either surface or the forms of the total distributions. Supposing from a *series* of consistent epidemics we have determined the forms of the "mildness of attack" or pseudo-resistance curves, we are no better off. To take an imaginary numerical illustration: Suppose these pseudo-resistance curves have unit standard deviations and that the distance between their means is also unity. We know nothing about the correlation. Assume that the standard deviation of the array (row) is 0·5. We know nothing about the s.d.'s

of the true resistance curves either; let us make them unity also. Then from the value of the s.d. of the array we have $1 - r^2 = 0{\cdot}25$, $r = 0{\cdot}8660$, and the second condition gives the slope of the regression line to the vertical as r. Hence the distance between the means of the true resistance distributions is $1/0{\cdot}8660$ or $1{\cdot}155$. The point round which the chance-of-recovery curve centres depends on the severity of the epidemic; suppose that k_0 in the pseudo-resistance curves is -1, then the chance-of-recovery curve centres round the deviation $-1/r$ or $1{\cdot}155$. The dispersion of the chance-of-recovery curve (cf. above, p. 144) is the s.d. of the array divided by r, or $0{\cdot}577$. Fig. 23 illustrates the case supposed. The two normal distributions drawn with unbroken

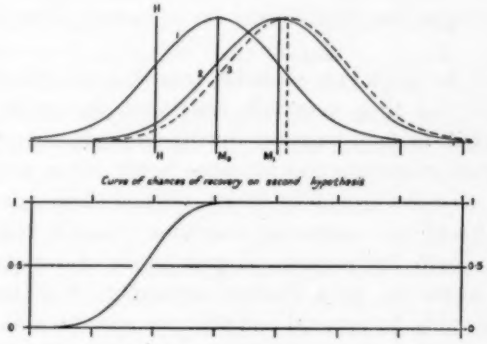


FIG. 23.

lines are the distributions arrived at by the method used in our illustrations. Retaining the same distribution as that assumed for the unvaccinated, the broken curve represents the true distribution for the vaccinated, the curve of chances of recovery being shown in the lower part of the figure. If the critical resistance for the first pair of distributions is moved by a unit, the centre of the chance of recovery curve must be moved by $1{\cdot}155$ units in the same direction. Precisely the same law will then be arrived at for relating the death-rates among vaccinated and unvaccinated.

It may, perhaps, be felt by the reader that the arguments of the preceding paragraphs have so mangled the original hypothesis that little vitality is left in it. We have shown, he may say, that the normal distributions which we have calculated may not represent the truth, for

the distributions may, in fact, be skew; and we have then proceeded to suggest that the calculation of the death-rates amongst immunized and unimmunized persons, by regarding the epidemic as eliminating all who possess less than a certain critical resistance, is a mere piece of mathematical jugglery. But such a pessimistic opinion we would by no means endorse.

We think it is in all probability essentially true that the resistances of immunized and non-immunized persons vary *inter se* and that these distributions may largely overlap. That, it seems to us, is the vital part of the hypothesis. We are, unfortunately, not in a position to make any direct determination of these distributions: they may very likely be asymmetric. It is an advantage, and not a disadvantage, if we can arrive at very much the same conclusions as regards the relative death-rates amongst the two classes on the supposition that they are normal.

Further, we do think it is probably true that the elimination of all individuals with less than a certain resistance cannot be regarded as representing what, in fact, happens during an epidemic, or by deaths in almost any other circumstances: chance must enter into the matter more or less. No one would be better pleased than ourselves if some way could be found for measuring resistance directly and determining the chance of death for persons of each grade of resistance. In the meantime it seems to us a distinct advantage that the element of chance may possibly be ignored and that we may be able to determine distributions of "pseudo-resistance" such that the elimination of individuals with pseudo-resistances less than R', $R' + d'$, $R' + 2d'$, $R' + 3d'$, &c., gives the same death-rates amongst the two classes compared, as would the application of the unknown law of chances of death to the true distributions of resistance, the law remaining unaltered in form but centring at values of the true resistance, R, $R + d$, $R + 2d$; and so on.

Section IV: The Measurement of the Relative Efficiencies of Different Immunization Processes.

We now come to the two questions which are of interest to the practical man—viz., granted that in certain cases, say cholera and typhoid, the death- or incidence-rates upon the uninoculated are higher than the corresponding values for the inoculated, and that these divergences cannot be dismissed as mere chance events, in which case

did the process of immunization produce the better result? If the typhoid results are good enough to entitle us to enforce inoculation in the case of troops moving into a typhoid-infected area, are the cholera results sufficiently good to authorize our taking the same step in the case of troops likely to be exposed to infection?

Professor Pearson answered such questions as these by referring to the values taken by his normal coefficient in the respective cases. Thus in his original memoir (1900) he obtained the value of 0·5954 for the correlation between what he termed "strength to resist small-pox when incurred" and "degree of effective vaccination." He then under the title "effectiveness of antitoxin treatment" prepared three tables for diphtheria, the horizontal dichotomy in each case being with "antitoxin, 1896" and "without antitoxin, 1894." The vertical dichotomies were (*a*) "recoveries" and "deaths"; (*b*) "requiring tracheotomy," and "not requiring it"; (*c*) "recoveries" and "deaths." The first two tables refer to laryngeal cases only: the third includes the cases of all children under five years of age. The normal coefficients in the three cases were 0·4708, 0·2385, 0·2451. Professor Pearson comments on these results as follows: "The three coefficients are all sensible as compared with their probable errors, and that between the administration of antitoxin and recovery in laryngeal cases is substantial. But the relationship is by no means so great as in the case of vaccination, and if its magnitude justifies the use of antitoxin, even when balanced against other ills which may follow in its train, it does not justify the sweeping statements of its effectiveness which I have heard made by medical friends. It seems until wider statistics are forthcoming a case for cautiously feeling the way forward rather than for hasty generalization" (op. cit. p. 45). We should conclude from this passage that Professor Pearson would rank the effectiveness of immunization processes in the order of the normal coefficients calculated from the available statistics, assuming, of course, that the differences are statistically significant.

We fear, however, that the questions propounded above cannot be answered in this simple fashion: a justification of this statement will be afforded by a closer examination of the problem. At the very outset one is faced with difficulties of definition. Thus, Professor Pearson speaks of the "effectiveness" of an immunization process. But this word may carry more senses than one. In what precise, numerical sense are we to understand it? To make the point clear, imagine the case of two diseases the fatalities of which were invariable, being

in every epidemic 50 and 5 per cent. respectively in the case of unimmunized persons. Now, suppose that in each case a process of immunization was employed which resulted in the fatality-rates of inoculated persons being reduced to 30 per cent. and zero respectively. In a certain sense the effectiveness of the immunization process in the second case is perfect, for the fatality-rate cannot be lower than zero. But, in another sense, the effectiveness is greater in the previous case because there the fatality-rate is reduced by 20 per cent. instead of 5 per cent. only. Evidently both these points of view are of importance. From the standpoint of the practical sanitarian a reduction of mortality from 50 to 30 per cent. is a greater achievement than a diminution from 5 per cent. to nothing. On the other hand, looking at the matter from the point of view of the student of immunizing processes, it might well be argued that the reduction to zero testifies to the establishment of a complete degree of immunity, the fact that the starting point was only 5 per cent. limiting the public advantage to be gained from the establishment of such a condition, but not its scientific interest. These considerations immediately suggest the desirability of employing two terms, in clearly defined senses.

We propose to define the *advantage* of an immunization process as the difference between the fatality-rates (or incidence-rates) of the unimmunized and immunized populations. The *efficiency* we propose to define as the ratio of the advantage to the fatality-rate (or incidence-rate) amongst the unimmunized—i.e., as the ratio of the numbers who are saved by the process to the numbers who might be saved. The practical application of these ideas is not, however, a simple matter. In our imaginary illustration we assumed that the fatality-rates of the two classes were invariable from epidemic to epidemic. But this is never the case, with the result that both measures vary from epidemic to epidemic of the same disease and that the figures for epidemics of two different diseases are not comparable. We may illustrate the first point on the cholera data. Assuming the two distributions of resistance of fig. 8, we have—

Incidence-rate	Advantage	Efficiency
0·9	0·6906	0·7673
0·8	0·6585	0·8231
0·7	0·5969	0·8527
0·6	0·5232	0·8720
0·5	0·4429	0·8858
0·4	0·3584	0·8960
0·3	0·2709	0·9030
0·2	0·1815	0·9075
0·1	0·0906	0·9060

It will be evident from this example that the "advantage" and the "efficiency" of any process of immunization are functions not only of the nature of the process, but also of the incidence-rate or fatality-rate. It hardly seems necessary to enforce the point by algebraic analysis. It follows that a comparison of the "advantages" or "efficiencies," when our data consist only of statistics relating to a single epidemic of each of two different diseases, may be seriously misleading. Were we in possession of a long series of epidemics in each case, the ranges of incidence- or fatality-rates being considerable, average values might reasonably be contrasted. Suppose, for example, fig. 24 represents the curve of p_2's for all values of p_1 (as in figs. 10 and 13), then RS is p_1, QR is $p_2 - p_1$ and PR is $1 - p_1$. The ratio of the area $AQCR$ to $ABCR$

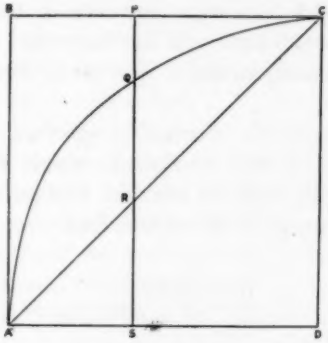


FIG. 24.

represents in a certain sense the mean efficiency, over the whole possible range of incidence- or fatality-rates. The area of the triangle ABC being 0·5, twice the area $AQCR$ is the mean efficiency. The area $AQCR$ itself is the mean advantage—i.e., the mean efficiency in this sense is twice the mean advantage. Applying this method to the case of swine fever and cholera (figs. 10 and 13), by a process of quadrature the mean efficiencies prove to be 0·404 and 0·799 respectively. These figures are not really comparable, since in one case we are dealing with deaths and in the other with attacks, but the example shows the sort of comparison that might have been made had the graphs referred to homologous data.

The practical limitations of the method are, however, apparent; not only is the actually observed range of incidence or fatality usually

very narrow, but we have assumed that the hypothesis discussed in the previous section is rigidly true and consequently permits of accurate extrapolation. Even when the data of several epidemics are available, the comparison of the efficiencies of two processes of immunization is attended with doubt and difficulty. When single epidemics are in question, the process becomes impossible. All we have then got is a single point on each of the two loci defining the inter-relations of the incidence- or fatality-rates of immunized and non-immunized persons.

If the soundness of the preceding arguments be admitted, it necessarily follows that no coefficient of correlation or association will furnish us with answers to the questions proposed at the beginning of this section. We believe that tentative answers can only be given on the basis of several epidemics, and with the help of some such process as that above described. In view, however, of the popularity enjoyed by certain of these coefficients and the fact that they have frequently been used in such investigations, it may be of interest to pass them in review.

(*A*) *Professor Pearson's Normal Coefficient.* — We have briefly described the nature of this coefficient, which has hitherto been by far the most frequently used, in the last section (pp. 141, 142). The fourfold table is assumed to be of the form—

		Variable X_1				
Variable X_2		$< H$		$> H$		Total
$< K$	...	a	...	d	...	$a + d$
$> K$	...	b	...	c	...	$b + c$
Total	...	$a + b$	...	$c + d$	...	N

and the coefficient of correlation is calculated on the assumption that the correlation between X_1 and X_2 is normal, the distribution being divided sharply into the four compartments by planes parallel to the axes of measurement. Now this coefficient might have been at once excluded, as one of us has previously pointed out (Yule, 1912, pp. 587-588), on the ground that it is not a function of p_1 and p_2 alone, but also of the proportion of vaccinated. For given values of p_1 and p_2 it is a maximum when the proportion of vaccinated is 50 per cent. Thus if the fatality-rate is 10 per cent. amongst the vaccinated and 40 per cent. amongst the unvaccinated, the normal coefficient is 0·57 when the proportion of vaccinated is 50 per cent., 0·50 when it is 15 per cent., 0·43 when it is 5 per cent., and only 0·35 when it is 1 per cent. If the proportion of

vaccinated be as high as 85 per cent. the coefficient falls from the maximum value to 0·53. What practical meaning can be assigned to a coefficient that behaves in such a way?

Further, if the arguments of the last section be admitted, the whole foundation of the method is cut away. For if the "vaccinated" and "unvaccinated" do not correspond to the mere dichotomy of one continuous distribution of resistance, but there is one distribution for the vaccinated and another for the unvaccinated, the two frequencies a and d constitute one frequency surface, the two frequencies b and c constitute another. There are no data for determining the correlation in either surface alone or in the two together. The correlation is, as we have stated above (p. 180), completely indeterminate.

It seems to us that in this respect Dr. Maynard did not fully realize the effect of his own hypothesis. After suggesting that the effect of inoculation was simply to shift the distribution of resistance to the right along the scale of "strength to resist attack," he merely concludes that "considering these difficulties . . . we cannot consider the 'fourfold' method as entirely satisfactory for our present purpose; nevertheless, if a correlation value is desired it is, I believe, the best method at present available for this purpose," and later remarks of the method that "although not perfect for this purpose it is useful as a control." Such observations read as if Dr. Maynard merely supposed that, on his hypothesis, the normal coefficient became a rather poor approximation to the true value of the correlation. But the coefficient appears to us to have become quite meaningless, for it purports to give a correlation which *cannot* be determined from the given data.

(*B*) *The Product-sum Correlation.*—This is the correlation coefficient (cf. Yule, 1912, pp. 595 *et seq.*)—

$$\begin{aligned} r &= \sqrt{\chi^2/N} \\ &= \sqrt{(p_1 - p_3)\ (p_2 - p_4)} \end{aligned}$$

where p_3 and p_4 are the proportions in the columns corresponding to p_1 and p_2 in the rows. Of this coefficient it is enough to remark that it varies with the number of the vaccinated even more rapidly than the normal coefficient. For the values of the fatality-rates assumed in the illustration given for that coefficient, the product-sum correlation falls from 0·35 when the proportion of vaccinated is 50 per cent. to 0·06 when the proportion is 1 per cent.

(*C*) *The Coefficient of Colligation* (Yule, 1912).—This coefficient, suggested by one of us a few years ago, may be expressed in terms

of the class-frequencies a, b, c, d, or in terms of p_1 and p_2, or in terms of p_3 and p_4 as follows:—

$$\omega = \frac{\sqrt{ac} - \sqrt{bd}}{\sqrt{ac} + \sqrt{bd}}$$

$$= \frac{\sqrt{p_1(1-p_2)} - \sqrt{p_2(1-p_1)}}{\sqrt{p_1(1-p_2)} + \sqrt{p_2(1-p_1)}}$$

$$= \frac{\sqrt{p_3(1-p_4)} - \sqrt{p_4(1-p_3)}}{\sqrt{p_3(1-p_4)} + \sqrt{p_4(1-p_3)}}$$

Since the coefficient can be expressed as a function of p_1 and p_2 only, it is evidently quite independent of the proportion of the population that is vaccinated, and is, therefore, not open to objection on the same grounds as the normal coefficient and the product-sum correlation. As a matter of fact, if the actual frequencies in the rows and columns of the observed table are multiplied by such coefficients that not only is the number of vaccinated made equal to the number of unvaccinated but also the number of recoveries made equal to the number of deaths, then the coefficient ω is the product-sum correlation for this derived symmetrical table. If the square-roots are omitted in the expression for ω, an even simpler expression (Q) is obtained which was given as a coefficient of association by one of us in 1900; it enjoys the same property as ω in being independent of alterations in the row or column totals, but is not so simply related to the product-sum coefficient and gives a less convenient scale.

Is this coefficient, then, any better than the others, seeing that at least it passes the elementary test? The answer must be, it is no better. It is quite right that the coefficient should be unaffected by altering the proportion of the vaccinated. For if we double or treble the area of the curve representing the distribution for the vaccinated in fig. 8 this will not (of course) alter the proportions of dead cut off to the left of the verticals 1, 2, 3, &c., in each curve. But the coefficient is *also* unaltered by varying the proportions of recoveries and deaths in the population, for this will leave the p_3 and p_4 of the table—the proportions of vaccinated amongst recoveries and deaths—unaltered, and this is *not* right on the theory developed in the last section. For if we alter the proportion of recoveries by shifting the critical vertical to the right, we *do* alter the proportions of vaccinated on the right of that vertical (amongst the recoveries) and on the left of that vertical (amongst the deaths), and we cannot help doing so. The coefficient is not constant therefore for varying inten-

sities of epidemic. The following table, calculated from the curves of fig. 8, shows the values of ω that would be obtained on our theory

Proportion of recoveries amongst uninoculated	Coefficient of colligation
0·1	0·7071
0·2	0·6625
0·3	0·6327
0·4	0·6188
0·5	0·6050
0·6	0·5934
0·7	0·5817
0·8	0·5691
0·9	0·5477

for successive proportions of recoveries amongst the uninoculated in the case of the cholera data utilized in illustration A of the last section. It will be seen that the coefficient falls steadily as the virulence of the epidemic decreases. There is no reason, of course, why a measure of efficiency should be constant over the range—we have seen in fact above that such is not the case; but the point is that we can assign no interpretation to the change, seeing that the coefficient assumes as a possibility what is, on our view of the facts, impossible. For the same disease and the same type of inoculation it is not, on our theory, possible to alter the proportion of deaths without altering the proportions of vaccinated among deaths and recoveries. The same arguments suffice to condemn the similar coefficient, Q.

This brief review does not, of course, cover all the possible coefficients that have been suggested, but will suffice to confirm the argument of this section. Our condemnation of these coefficients, for the present purpose, may at least claim to be impartial, inasmuch as two of them were originally proposed by one of us.

The results of this section are disappointing in so far as they fail to provide a simple answer to important practical questions. On the other hand, we venture to hope that they will be of value to subsequent inquirers. The general lesson to be learned is that mathematical difficulties of method must not absorb the whole energies of the statistician. To Professor Pearson and his pupils we owe the solution of many mathematical difficulties, but Dr. Brownlee and Dr. Maynard alone, so far as we are aware, have assigned a due measure of importance to the biological difficulties of interpretation which present themselves in connexion with such inquiries as that on which we have been engaged.

BIBLIOGRAPHY.

Antityphoid Committee, Report of, 1912.

Brownlee, J. "Statistical Studies in Immunity," *Biometrika*, 1905, iv, p. 313.

Idem. "The Causes of Epidemics and the Laws which regulate their Course," *Proc. Sect. XVIII Internat. Congress of Med.*, 1913.

Idem. "On the Curve of an Epidemic," *Brit. Med. Journ.*, 1915, i, p. 799.

Cardamatis, J. P. "Rapport sur la lutte contre le choléra en Macédoine pendant la guerre Gréco-Bulgare," *Bull. de la Soc. de Path. Exot.* 1914, vii, p. 447.

Firth, R. H. "A Statistical Study of Anti-enteric Inoculation," *Journ. Roy. Army Med. Corps*, 1911, xvi, p. 589.

Greenwood, M., jun. "The Factors that determine the Rise, Spread, and Degree of Severity of Epidemic Disease," *Proc. Sect. XVIII Internat. Congress of Med.*, 1913.

Haffkine, W. M. "Protective Inoculation against Cholera," Calcutta, 1913.

Macdonnell, W. R. "On the Influence of Previous Vaccination in Cases of Small-pox," *Biometrika*, 1901, i, p. 375; 1902, ii, p. 135.

Maynard, G. D. "Statistical Studies of Anti-typhoid Inoculation," *Biometrika*, 1908, vi, p. 366.

Idem. "An Enquiry into the Etiology, Manifestations, and Prevention of Pneumonia amongst Natives on the Rand recruited from Tropical Areas," Johannesburg, 1913. (South African Institute for Medical Research.)

Moutouses, K. J. "Die Cholerabekämpfung i. d. griechischen Armee während des griechischen-bulgarischen Kriegs," *Militärarzt*, 48 J., p. 66. (Supplement to No. 10, *Wien. med. Wochenschr.*, 1914.)

Murata, N. "Ueber die Schutzimpfung gegen Cholera," *Centralbl. f. Bakt.* (Orig.), 1904, xxxv, p. 605.

Nijland, A. H. "Eenige resultaten met het Cholera-vaccin verkregen," *Geneeskundig Tijdschrift v. Nederlandsch-Indië*, 1911, Deel 51, afl. 4, and 1913, Deel 53, afl. 1.

Pearson, K. "On the Criterion that a Given System of Deviations from the Probable in the Case of a Correlated System of Variables is such that it can be reasonably supposed to have arisen from Random Sampling," *Phil. Mag.*, July, 1900.

Idem. "On the Correlation of Characters not Quantitatively Measurable," *Phil. Trans.*, A, 1900, cxcv, p. 1.

Pearson, K., and Heron, D. "On Theories of Association," *Biometrika*, 1913, ix, p. 159.

Powell, A. "Results of Haffkine's Anti-cholera Inoculation," *Lancet*, 1896, ii, p. 171.

Idem. "Further Results of Haffkine's Anti-cholera Inoculation," *Journ. Trop. Med.*, 1899, vi, p. 115.

Ross, Sir R. "Some *a priori* Pathometric Equations," *Brit. Med. Journ.*, March 27, 1915, p. 546.

Savas, C. "Ueber die Choleraschutzimpfung in Griechenland," *Wien. klin. Wochenschr.*, 1914, xxvii, p. 1093.

Simon, P. L.; Pasteur Vallery-Radot; Kiamil Bey; Asseo. "Notes sur le Choléra à Constantinople et en Thrace de 1910 à 1913," *Bull. de la Soc. de Path. Exot.*, 1914, vii, p. 313.

Sticker, C. "Die Cholera," Giessen, 1912.

Idem. "Ueber die Bedingungen, von denen die Entwicklung, die Ausbreitung und die Schwere der Epidemien abhängen," *Proc. Sect. XVIII Internat. Congress of Med.*, 1913.

Yule, G. U. "On the Association of Attributes in Statistics," *Phil. Trans.*, A, 1900, cxciv, p. 257.

Idem. "On the Methods of Measuring Association between two Attributes," *Journ. Roy. Stat. Soc.*, 1912, lxxv, p. 579.

Zabolotny. "Schutzimpfung gegen Cholera," *Bericht u. d. 14ten Congress f. Hygiene z. Berlin*, 1907.

Idem. "L'Epidémie du Choléra 1907-08 en Russie," *Arch. d. Sci. Biol.*, St. Petersb., 1909, xiv.

DISCUSSION.

Dr. W. H. HAMER said he was very sorry to be occupying the Chair, as his doing so was caused by the absence of the President; but he was very glad to have enjoyed the pleasure of being present, when the extremely interesting paper they had just listened to was read. The great advances which were being made year by year in the sciences of bacteriology and statistics made the lot of the medical officer of health anything but a happy one. On the last occasion when he was present at a meeting of the Section, they were given marching orders with regard to cerebrospinal meningitis. It was a comparatively simple matter to find a meningococcus carrier, but what was to be done with him when he was found. However, he would not enlarge upon that topic, for fortunately the outbreak of cerebrospinal fever was abating, and clearly he had better not attempt to estimate how far this was due to restraining the movements of healthy carriers, and how far it was due to the natural course of events. With regard to typhoid prevention, two points connected with typhoid fever in London particularly struck him: *first*, the immunity of the London population from the disease—in one or two recent weeks the number of cases had fallen as low as five, a very small number for so large a community; and, *secondly*, even now, there was very great uncertainty as to the diagnosis of typhoid fever, for the percentage of errors of diagnosis in cases sent to the Metropolitan Asylums Board Hospitals amounted to nearly forty. If such difficulties with regard to diagnosis were experienced in London, he wondered whether similar difficulties might not also be experienced elsewhere. One question he would particularly like to ask Mr. Greenwood, as it possibly might have a bearing upon the statistical theory developed in Section III of the paper. In the case of swine fever, inoculation was now being practised with blood which it was presumed contained the filter-passer cause of the disease: some work had been done, however, in years gone by, with cultures of the hog cholera bacillus, an organism which was at that earlier period actually believed to be the cause of hog cholera. It would be extremely interesting, if it were possible, to make comparison between the results obtained by employing these two methods: *first*, in respect of protection against attack; and *secondly*, in respect of protection against a fatal result. It might, perhaps, be suggested, on merely theoretical grounds, that cultures of an "associated organism" were more likely to protect against a fatal result than against attack by the disease; while inoculation with the filter-passing causal organism might perhaps be expected to protect, if at all, rather against attack than against severity of illness. If Mr. Greenwood, who was particularly interested in the subject of swine fever, could give them any enlightenment on these points, the speaker was sure they would all be grateful, for if it was possible to investigate the facts, so far as swine fever was concerned, on these lines, the results obtained might then be considered in their bearing upon the very difficult case of typhoid fever. He

was assuming that everything was not at the present time known about the causal organism of typhoid fever, a supposition which some high authorities seemed inclined to doubt.

The discussion was continued by Dr. E. W. GOODALL, Dr. BROWNLEE, Dr. G. S. BUCHANAN, and Dr. J. C. MCVAIL.

Professor W. J. R. SIMPSON, C.M.G., said that the authors were to be congratulated on their very able paper. The array of assumptions and figures in the paper were perplexing to one who was not a mathematician, but it was obvious to all that Mr. Major Greenwood and Mr. Yule had approached the subject with an open and thoroughly critical mind and had dealt with it in a most comprehensive manner. He was glad to find that the result of their examination of the statistics was the conclusion that the Calcutta, Assam, and Java statistics, together with those relating to the Sanitary Corps of the Greek Army, afforded sufficient ground for the introduction of cholera inoculations in the British Army as a prophylactic against cholera. Though cautiously expressed, it was an important pronouncement, and one with which he agreed. In fact, his own experience led him to advocate the prophylactic measure much more strongly than the writers of the paper. He thought that no time should be lost in protecting the soldiers, especially in Egypt and the Dardanelles, against this disease. He would like to point out that owing to the favourable conditions under which the Calcutta observations were made and the special care taken to secure their accuracy, the figures, though small in numbers, were more significant than they appeared, for they represented over seventy distinct and separate experiments, not during the period of a single epidemic but extending over more than two years in an endemic locality. Cholera was endemic in Calcutta. It was more prevalent in the early and later months of the year than at other times, but there was no month in which the town was free of the disease. There were certain parts of the town in which the disease recurred more frequently than in others, and it was here that most of the 7,000 inoculations were carried out. The method adopted was as far as possible to inoculate half the members of a household and leave the other half not inoculated, and then await events. During the period under observation cholera occurred in many of the houses where no inoculations had been performed. These cases were of no special interest. But cholera also entered some seventy houses where part of the household had been inoculated and the remainder had not been inoculated. It was remarkable to note how, after the fourth day of the inoculation, when immunity had been established among the inoculated, cholera picked out in these houses the non-inoculated and left the inoculated unharmed. This went on repeating itself month after month to such an extent that everyone connected with watching the experiments was deeply impressed by the protective value of the inoculations, the protection lasting for over fourteen months. Even the inhabitants of these areas noticed the difference of the incidence of cholera among the inoculated and non-inoculated and came forward to be inoculated, many insisting on all the members of the household being given the prophylactic.

Mr. YULE, replying to some of the theoretical points raised in the discussion, said that he had listened with much interest to the remarks made by Dr. Brownlee and was glad to know that that speaker was in general agreement with their views. He well remembered Dr. Brownlee's statement as to the distinctness of small-pox in the case of vaccinated and unvaccinated patients. On this point Mr. Greenwood and himself were rather inclined to agree with Dr. Buchanan: if the diseases in the two cases were almost specifically distinct their hypothesis would hardly apply. They must be sufficiently similar to enable one to plot the two curves of resistance to the same scale—i.e., the difference must be of a quantitative rather than of a qualitative kind. He would like to know more of Dr. Brownlee's views on this head. Dr. Brownlee had also called attention to the frequency of linear regression in statistical work. They agreed that such regression was frequent, and might perhaps add that, for this reason, they could not regard the closeness with which their hypothesis was fulfilled as any strong evidence of its truth. The hypothesis must at present rest rather on the *a priori* evidence that it was a natural and reasonable assumption than on any support afforded by statistics. Professor Simpson had referred to the caution of their conclusions as regards cholera inoculation, and the conviction that had been carried to his own mind by actual experience when he had been able to note the experience of inoculated and uninoculated persons in the same house. They had been compelled, of course, to deal with the data in a summary way and to point out the imperfections of many of the data presented, but he did not think that there was any doubt about their final conclusion, especially when the data of Savas and of Haffkine were considered together. Where they did find themselves in real difficulties was on a different point—namely, when they tried to compare the efficiency of the immunization process in the case of cholera with that of the immunization process in the case of another disease. Of this they were unable to provide a statistical measure. With the remarks made by Dr. McVail he was in complete agreement. Dr. Buchanan had commented on a passage respecting the distribution of "effective vaccination." It was, of course, not quite clear what Professor Pearson understood by that term: it had to be interpreted in the sense of a variable magnitude that could be plotted on a scale, and it then seemed to them to be almost identical with resistance. In any case, it did seem to them that a person with no cicatrix recorded might be more "effectively vaccinated" than a person with a very faint cicatrix; or, if he had had a previous attack of small-pox, than a person even with a well-marked cicatrix. Mathematicians were just as likely to be wrong as other people were, and data drawn from any particular science could not be adequately treated by the mathematician unless he was as much of an expert in that particular science as he was in mathematics. He himself would not have attempted to write such a paper as the present alone; fortunately, he had been associated with Mr. Greenwood.

Mr. MAJOR GREENWOOD, jun., desired merely to express his complete concurrence with respect to points his colleague had mentioned in replying

on the discussion. He might emphasize Mr. Yule's remark that they both intended to convey the conclusion that, so far as the data went, the case in favour of anti-cholera inoculation seemed to them strong. The question as to which arrangement of Haffkine's material should be chosen had been considered, and he believed that that adopted in the paper was the most satisfactory. A separate consideration of the incidence of disease upon individual houses would not, he thought, have led to the expression of any more pointed conclusions.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE EIGHTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1914-15

SECTION OF THE HISTORY OF MEDICINE

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1915

Section of the History of Medicine.

OFFICERS FOR THE SESSION 1914-15.

President—

NORMAN MOORE, M.D.

Vice-Presidents—

HENRY BARNES, M.D.
RICHARD CATON, M.D.
Sir HENRY MORRIS, Bt., F.R.C.S.
Sir WILLIAM OSLER, Bt., M.D., F.R.S.
Sir RONALD ROSS, K.C.B., F.R.S.
Sir FREDERICK TREVES, Bt., G.C.V.O., C.B., F.R.C.S.

Hon. Secretaries—

RAYMOND CRAWFURD, M.D.
LEONARD G. GUTHRIE, M.D.

Other Members of Council—

DUDLEY W. BUXTON, M.D.
Sir JOHN WILLIAM BYERS, M.D.
A. J. CHALMERS, M.D.
F. W. COCK, M.D.
ALBAN DORAN, F.R.C.S.
DAVID FORSYTH, M.D.
JAMES GALLOWAY, M.D.
E. MUIRHEAD LITTLE, F.R.C.S.
R. O. MOON, M.D.
Sir SHIRLEY F. MURPHY, F.R.C.S.
J. A. NIXON, M.B.
GEORGE C. PEACHEY.
D'ARCY POWER, F.R.C.S.
F. M. SANDWITH, M.D.
CHARLES SINGER, M.D.
GEORGE W. STEEVES, M.D.
C. J. S. THOMPSON.
P. MACLEOD YEARSLEY, F.R.C.S.

Representative on Library Committee—

RAYMOND CRAWFURD, M.D.

Representative on Editorial Committee—

LEONARD G. GUTHRIE, M.D.

SECTION OF THE HISTORY OF MEDICINE.

CONTENTS.

November 18, 1914.

PAGE

Joseph Ignace Guillotin. By Macleod Yearsley, F.R.C.S. 1

Oliver Goldsmith and Medicine. By Raymond Crawfurd, M.D.Oxon.... ... 7

December 16, 1914.

400th Anniversary of Vesalius 27

A Westmorland Medical Superstition. By G. C. Peachey 27

The Apothecary in England from the Thirteenth to the Close of the Sixteenth Century. By C. J. S. Thompson 36

January 20, 1915.

Exhibition of Books illustrating the History of Military Hygiene 45

Lucian and Medicine. Part I. By J. D. Rolleston, M.D. 49

February 17, 1915.

Jean Astruc. By Emeritus Professor Sir Alexander Simpson, M.D., D.Sc., LL.D. 59

Lucian and Medicine. Part II. By J. D. Rolleston, M.D.... 72

March 17, 1915.

PAGE

William Withering, M.D., F.R.S. By A. R. Cushny, M.D. 85

Thomas Baynton, 1761-1820. By J. A. Nixon, M.B. 95

May 19, 1915.

William Bromfield, 1713-1792. By G. C. Peachey 103

Lionel Lockyer. By Hector A. Colwell, M.B.Lond. 126

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons and Danielsson, Ltd., 83-91, Great Titchfield Street, London, W.

Section of the History of Medicine.

November 18, 1914.

Dr. Norman Moore, President of the Section, in the Chair.

Joseph Ignace Guillotin.

By Macleod Yearsley, F.R.C.S.

The document which I show at this meeting dated "Paris, le 23 Germinal 4me année Republicaine," is signed by one of the most interesting of the many persons who figured in the Revolution—Dr. Joseph Ignace Guillotin. "Worthy Doctor Guillotin, respectable practitioner in Paris," as Carlyle describes him, and of whom he says, "This respectable Guillotin we hope to behold once more, and perhaps only once. . . . Doomed by a satiric destiny to the strangest immortal glory that ever kept obscure mortal from his resting-place, the bosom of oblivion." This document is not one of any great importance, merely dealing with the disposal of the effects of the many arrested by the Directory and guillotined on the 7 Ventôse. It offers an opportunity, however, for mentioning the evidence which refutes the error, not infrequently perpetuated in books and articles, and which appears to be shared by quite a number of people, that Guillotin, by the irony of fate, perished by his own invention.

As a matter of fact, Guillotin survived the Revolution, and died on March 26, 1814, just over one hundred years ago. He was a remarkable personage in his way. He had witnessed the fall of the French monarchy, the massacre of August 10, 1792, and the still more dastardly murder of the defenceless prisoners in the following month of September.

He had been in deadly peril during the latter part of the Reign of Terror; had witnessed probably the closing scene in the lives of Robespierre, Saint-Just and Couthon, with whom that awful Reign of Terror ceased to exist. He had passed through the subsequent rule of the Directory, had witnessed the *coup d'état* of the 18 Brumaire, and the successive dominations of the Consulate and the Empire which succeeded.

The evidence which is extant to prove his death in 1814 is the certificate which was issued in conformity with the law of the period when he died, the text of which, translated, is as follows:—

"On the 26th of March, one thousand eight hundred and fourteen, at three o'clock in the afternoon.

"Certificate of the registry of death of Monsieur Joseph Ignace Guillotin, who died the day aforesaid, at three o'clock in the afternoon, at No. 533, rue Saint Honoré, quarter of the Tuileries, doctor of medicine, aged about seventy-six years, born at Saintes, department of la Charente Inférieure, and married to dame Marie Louise Saugrain.

"Verified by us, Antoine Charles Roye, mayor of the first arrondissement of Paris, performing the duties of officer of the civil state, upon a declaration made to us by Messieurs Joseph-Raymond Plassan, printer, of the age of thirty-one years, of No. 17, rue Vaugirard, nephew of the deceased, and Augustin Pierre Rousseau, ecclesiastic, of the age of fifty-three years, residing at No. 18, rue de Foin-Saint Jacques.

"And signed by us, after having been first read.

"PLASSAN, ROUSSEAU, ROYE."

Guillotin was the son of Joseph-Alexandre Guillotin, advocate, and was born at Saintes on May 28, 1738. He went through a course of preliminary study at Bordeaux, and took his degree of Master of Arts at the College of Acquitaine, on or about December 11, 1761. His early years were spent among the Jesuits, and he taught for some years at the Irish College of Bordeaux. In spite of the efforts made by the Jesuits (attracted, according to Dr. Achille Chereau, by his name of *Ignatius*), his inclinations led him to take up the profession of medicine, and he became a Doctor of Medicine at the College of Rheims on January 7, 1768. He then studied at the School of Medicine in Paris, preparatory to practising in the capital. Not being overburdened with means, he applied to the Dean, Pierre Bercher, for accommodation in one of the many small houses possessed by the old School of Medicine

I.re DIVISION.

2.e *Section.*

Nota. Mettre en marge de la réponse les mots ci-dessus.

LIBERTÉ. ÉGALITÉ.

Paris Le 23 Germinal 4.e année Républicaine

LE BUREAU DU DOMAINE NATIONAL
du Département de ~~Paris~~ La Seine

Au Citoyen Chaumet Son Commissaire.

Le Ministre des finances par sa lettre du 13 de ce mois, Citoyen, en réponse à celle que nous lui avons adressée le 22 Ventose dernier relativement aux mesures que nous avons prises pour l'exécution de l'arrêté du directoire Exécutif du 7 du même mois, nous observe à l'égard des draps, linge de table et de cuisine qui doivent être exceptés des Ventes, que le but de la réserve qui en est ordonnée étant d'employer ces objets aux ameublements autorisés par la constitution, on ne doit en conséquence réserver que ceux qui est bon et en état de Servir. mais il nous recommande en même tems de Veiller à ce que nos comm.rs ne s'autorisent pas de cette distinction pour Vendre du linge sous prétexte qu'il est mauvais.

Vous voudrez bien vous conformer aux intentions du Ministre et nous accuser la réception de la présente.

Salut et fraternité.

Le Commissaire Guillotin

in its immediate neighbourhood, and secured his wish at the modest rental of 324 livres (about £3 10s.) per annum. On February 27, 1768, he won a scholarship, and two years later, on August 27, 1770, became a Licentiate, finally receiving his cap as Doctor of Medicine on October 26 of the same year.

For the next fourteen or fifteen years his life was devoted to the practice of his profession, in which he established a reputation. It was in 1778 that he served, with five members of the Academy and three other physicians, on the Commission on Mesmerism, and contributed not a little to the exposure of that cult and the exit of Mesmer from France.

At the outbreak of the Revolution, Guillotin adopted the new principles, and his sympathy was by no means of a passive character. His *Pétition des Six Corps*, which was extensively signed by the middle classes of Paris, got him into trouble, and he was summoned to the Parlement of Paris in December, 1788, but was acquitted and brought back in triumph by the people. His *pétition* began his short political career, and at once brought him into notoriety. On April 26, 1789, the electors, under the presidency of Target, named Bailly and Guillotin secretaries, and on May 5 he took his seat at Versailles as tenth deputy for Paris.

By the end of September, 1791, Guillotin quitted political life and resumed the exclusive practice of his profession. Before dismissing him to his comparatively private life, a short note must be given of the circumstances which led to the invention which, again to quote Carlyle, kept him from the "bosom of oblivion." On October 9, 1789, the National Assembly began a discussion upon the reform of criminal jurisprudence. The important changes which followed this discussion need not be described, save that it abolished the "question," and declared that sentence of death, without appeal, should be pronounced only on the votes of four-fifths of the presiding judges. Up to this time, death was inflicted invariably by hanging, unless the condemned was of noble blood, when he was entitled to the questionable "privilege" of decapitation, inflicted by means of the sword. On December 10, 1789, Guillotin propounded six articles which he had drawn up. Four of these proposed:—

(1) Similar offences to be punished by similar punishment, without reference to rank or condition.

(2) Offences and crimes being purely *personal*, the punishment should cast no slur upon the family of the criminal.

(3) Confiscation of the goods of the condemned in no case to be pronounced.

(4) The body of an executed person to be given up to his family on demand.

These four articles only were adopted of the original six, and, in his speech, Guillotin advised that all executions should be by decapitation, and should be carried out by a "simple machine." This instrument he described, saying, "My machine acts like lightning; the head flies; the blood gushes out; the man is no longer there."

For some time Guillotin's "machine," which, it must be insisted, was suggested purely from motives of humanity, was regarded as an amiable hobby, and nothing further was done. In 1791 it was proposed to abolish the punishment of death altogether. The discussion in the National Assembly on this suggestion is remarkable chiefly for the fact that Robespierre pleaded earnestly for the absolute abolition of the death penalty in *all* cases, and Guillotin sat silent on his bench. The proposition was rejected, and the method of execution was referred to the Committee on Legislation, which recommended decapitation. To make a long story short, after much consultation between Sanson, the executioner, a German maker of musical instruments, named Schmidt, Antoine Louis, the surgeon, and Guillotin, who was called in for the purpose from his private retirement, the guillotine was evolved. The first instrument was made by one Guédon, a carpenter and joiner, who furnished an estimate of 5,660 livres (about £235 11s. 8d.). It was tested at Bicêtre on five dead bodies on April 15, 1792. Whether Guillotin was present is doubtful. He had retired from political life, and the realisation of his "idea" had for all practical purposes been taken out of his hands, and the probability is that he kept away. The result of the trial was satisfactory, and the guillotine's first victim was Nicholas Jacques Pellétier, executed on April 25, 1792, for a sanguinary murder and robbery. The mob testified its disapproval; like most mobs it soon veered round! The surgeon, Antoine Louis, appears to have played the chief part in the realisation of Guillotin's "idea"; indeed, according to Duplan, it was originally called the "*Petite Louison*" and is still known by the name of "*Louisette.*" It remains, therefore, to explain how it obtained the title by which it immortalised its real originator. Guillotin and his hobby were the butt of many sarcasms, and in 1789, the year in which he propounded his suggestion, a song appeared in a Royalist sheet called *Les Actes des Apôtres.* The subject of this song was "L'inimitable machine du Médecin Guillotin,

propre à couper les têtes, *et dite de son nom, Guillotine.*" To that song, according to M. Reynard, the "realised idea" owes its name. This song was as follows:—

"Guillotin,
Médecin
Politique,
Imagine, un beau matin,
Que pendre est inhumain
Et peu politique.

Aussitôt
Il lui faut
Un supplice
Qui, sans corde ni poteau,
Suprime le bourreau
D'office.

C'est en vain que l'on publie
Que c'est pure jalousie
D'un suppôt
Du tripot
D'Hippocrate,
Qui d'occire impunément,
Même exclusivement,
Se flatte.

Le Romain
Guillotin,
Qui s'apprête,
Consulte gens de métier,
Barnave et Chapelier,
Même le Coupe-tête;
Et sa main
Fait sudain
La machine
Qui simplement nous tuera,
Et que *l'on nommera*
Guillotine."

On May 20, 1792, little more than a month after the trial of the guillotine at Bicêtre, Antoine Louis fell under its knife. Guillotin, himself, became a "suspect," and narrowly escaped the same fate, but he was imprisoned at a favourable time, close to Thermidor, and the prison doors were opened for him so soon as the heads of Robespierre and his fellow-triumvirs had fallen. He lived to be President of the *Comité* de Vaccine, and was respected as a benevolent physician.

Oliver Goldsmith and Medicine.

By Raymond Crawfurd, M.D.Oxon.

The relations of Oliver Goldsmith with the profession of medicine, and the vexed question of his medical graduation, have recently been the subject of two illuminating papers[1] by Sir Ernest Clarke. On the strength of these he asks us to accept it, as established beyond reasonable doubt, that Goldsmith obtained the degree of Bachelor of Physic (M.B.) from Dublin at some date prior to 1763. Believing, as we do, that the evidence is compatible only with the presumption that Goldsmith had no degree in medicine at all, other than that conferred by Oxford University in 1769, probably under a misapprehension, we venture to indicate some of the difficulties that seem to need explanation before acceptance of his conclusion. Perhaps from the autograph letters and intimate memoranda, with mention of which he has tantalised but not gratified us, he may produce other information to substantiate his claim. Dates are of such material importance to the inquiry that it is necessary at the outset to determine a few prominent landmarks in Goldsmith's career.

Born on November 10, 1728, he was admitted a sizar at Dublin on June 11, 1745, and took a belated degree of Bachelor of Arts, also at at Dublin, on February 27, 1749, at the age of 20. After this, still resident all the time in Ireland, he successively read for holy orders, held a private tutorship for a year, coquetted with the profession of law, and wasted much time and money in indolence and adventure, till some date in 1752, when the family oracle, Dean Goldsmith, of Cork, propounded the opinion that Providence had fashioned him for nothing else than a doctor. The first-fruits of this advice seem to have been the attendance of Oliver at some courses of anatomy in Dublin. This is affirmed by Glover, who himself commenced the study of medicine in Dublin, and had therefore every opportunity of knowing. Be it noted also, in passing, that this same Glover states that it was at Louvain that Goldsmith obtained his medical degree. For some reason which we do not know, but which in view of the record of his previous turbulent residence in Dublin we may guess, it was decided that the heavier soil of Edinburgh would be more favourable to the growth of

[1] "Oliver Goldsmith as a Medical Man," *Nineteenth Century and After*, April, 1914; "The Medical Education and Qualifications of Oliver Goldsmith," *Proc. Roy. Soc. Med.*, 1914, vii (Hist. Sect.), pp. 88-98.

this very capricious sapling. So to Edinburgh he went in the autumn of 1752. Of his presence in this medical school, at the end of October, 1752, there is documentary evidence in the extant class-rolls of the professor of anatomy. At this point the known facts seem to conflict with the conclusions of Sir Ernest Clarke's papers. Among the memoranda of Bishop Percy, made from Goldsmith's own dictation on April 28, 1773, and recently in Sir Ernest's temporary possession, is the following:—

"After taking the degree of A.B. he proceeded upon the line of Physic, and took the degree of M.B. when he was about 20, he however ceased to reside after his degree of A.B."

Now we have seen that Goldsmith did not commence the study of medicine till between 23 and 24 years of age; therefore, if we are to accept his statement as true, we must assume that Dublin conferred its medical baccalaureate upon him before he had commenced the study of medicine, after a career which had given a good deal of incidental dissatisfaction to the University authorities, without further residence, and without lapse of the prescribed interval between the Arts and the medical degrees. Now when Goldsmith made this statement to Percy in 1773, he had no option but to specify Dublin as the source of his original medical degree, for he had formally notified the same to Oxford University in 1769, when Johnson and Percy accompanied him thither for the conferment of the *ad eundem* degree of that University. No one reading fairly Goldsmith's statement as given by Percy can contend that Dublin is not indicated, seeing that, until he was close upon the age of 24, Goldsmith had never left Ireland, and had had no relations whatever with any university but that of Dublin, where also he had graduated B.A. We know, however, under Goldsmith's own hand, from an undated letter to his uncle Contarine, written from Leyden early in 1754, that he was without a degree at the age of 25. We need not press this point further at this stage beyond insisting that either Goldsmith's statement or Percy's record of it is wrong, and, not unmindful of Johnson's testimony to Percy's meticulous accuracy, we shall hope to show that the former alternative accords best with the other evidence. Loyalty to his friend, whose frailties were only too familiar to him, appears more than once to move Percy to an economy of information.

Of Goldsmith's life in Edinburgh his own surviving letters tell us enough to justify the belief that, at that period of his career, he did

seriously intend to prepare himself for the practice of medicine. On May 8, 1753, he wrote to his uncle Contarine, as follows:—

"Apropos, I shall give you the professors' names, and as far as occurs to me, their characters: and first, as most deserving, Mr. Munro, professor of Anatomy. This man has brought the science he teaches us to as much perfection as it is capable of; and not content with barely teaching anatomy, he launches out into all the branches of physic, when all his remarks are new and useful. 'Tis he, I may venture to say, that draws hither such a number of students from most parts of the world, even from Russia. He is not only a skilful physician, but an able orator, and delivers things in their nature obscure in so easy a manner, that the most unlearned may understand him. Plume, professor of Chemistry, understands his business well, but delivers himself so ill, that he is but little regarded. Alston, professor of Materia Medica, speaks much, but little to the purpose. The professors of Theory and Practice (of physic) say nothing but what may be found in books laid before us; and speak that in so drowsy and heavy a manner, that their hearers are not many degrees in a better state than their patients. You see then, dear sir, that Munro is the only great man among them, so that I intend to hear him another winter, and go then to hear Albinus, the great professor at Leyden. I read (with satisfaction) a science the most pleasing in nature, so that my labours are but a relaxation, and, I may truly say, the only thing here that gives me pleasure. How I enjoy the pleasing hope of returning, with skill, and to find my friends stand in no need of my assistance," &c.

That Goldsmith did actually carry out his intention of sitting at the feet of Munro for another winter session we learn from the extant class-rolls of that professor.

An entry in the books of the Medical Society of Edinburgh, a voluntary association of students, shows that Goldsmith was admitted a member on January 13, 1753, but was exempted from the usual obligation of reading a paper on a medical subject; perhaps his social rather than his professional recommendations had secured him the entrée of the society. We may assume that the child was father of the man, for Percy speaks of his inordinate desire throughout life to gain applause by buffoonery. Goldsmith did not find to his liking the motherly frugality of his Edinburgh landlady, who made a leg of mutton, variously garnished, do duty for a week, the residual bone functioning as the chief ingredient of a broth on the seventh day, and took himself off to a lodging frequented by other students, whom he entertained with songs and stories. Percy says that his attention to his studies at Edinburgh was by no means regular, and that his health was injured and his pocket drained by his mixing in orgies of dissipation.

An extant leaf of his Edinburgh tailor's ledger testifies to his weakness for gaudy clothing, but at the same time exonerates him, at this period of his life, from the charge of extravagant expenditure on dress. Such other knowledge as we possess of his doings at Edinburgh is not essential to our subject, but it will be well to note that he was habitually short of money, and that supplies, necessarily small, were forwarded to him periodically by his uncle Contarine.

From Edinburgh, after sixteen months' residence, Goldsmith transferred himself, in February, 1754, to Leyden, and it will be necessary to investigate the circumstances of this transference. The first material document is a letter to his uncle, the Rev. Thomas Contarine, written round about the end of 1753, as follows:—

"My dear Uncle,—

"After having spent two winters in Edinburgh, I now prepare to go to France the 10th of next February. I have seen all this country can exhibit in the medical way, and therefore intend to visit Paris, where the great Mr. Farhein, Petit, and Du Hammel de Monceau instruct their pupils in all the branches of medicine. They speak French, and consequently I shall have much the advantage of most of my countrymen, as I am perfectly acquainted with that language, and few who leave Ireland are so. Since I am upon so pleasing a topic as self-applause, give me leave to say that the circle of science which I have run through, before I undertook the study of physic, is not only useful, but absolutely necessary to the making a skilful physician. Such sciences enlarge our understanding, and sharpen our sagacity; and what is a practitioner without both but an empiric, for never yet was a disorder found entirely the same in two patients. A quack, unable to distinguish the particularities in each disease, prescribes at a venture: if he finds such a disorder may be called by the general name of fever, for instance, he has a set of remedies which he applies to cure it, nor does he desist till his medicines are run out, or his patient has lost his life. But the skilful physician distinguishes the symptoms: manures the sterility of nature or prunes her luxuriance: nor does he depend so much on the efficacy of medicines, as on their proper application. I shall spend this spring and summer in Paris, and the beginning of next winter go to Leyden. The great Albinus is still alive there, and 'twill be proper to go, though only to have it said that we have studied in so famous an university."

Of this letter it may be said that it would be difficult to find in the most thoughtful medical literature of the time so enlightened a plea for rational therapeutics as against the penny-in-the-slot practice of medicine, which was then almost universal: nor so clear an appreciation of the double rôle of medicine, as a science of the sciences on the one hand and as an art on the other. It is the letter of a man whose

horizon is broad, and the foundations of whose knowledge have been laid broad and deep, and shows no trace of lack of earnestness in the serious pursuit of his profession. One almost wishes that Percy had interjected fewer emendatory glosses, and sister Hodson too, who throws light on the circumstance of his deliberately courting the increased expenditure of studentship at foreign universities at a time of severe pecuniary straits, when she speaks of his native restlessness and inborn love of travel—a passion that, under favouring conditions, was destined very soon to become the dominant influence.

On leaving Edinburgh, in February 1754, Paris, and not Leyden, was apparently his immediate objective. A letter, written a few weeks later from Leyden, says that he set sail on a ship bound for Bordeaux, but was driven by a storm into Newcastle, where he was arrested, as his ship-mates were found to be Scotchmen who had been enlisting recruits for the French army in Scotland. After a fortnight in prison he was able to disprove his complicity and was set at liberty, but meantime the ship had proceeded on its journey which was doomed to end in a disastrous wreck. In this plight he was constrained to take such passage as offered, which was, as he tells us, in "a ship at that time ready for Holland": so from Newcastle he sailed to Rotterdam, and travelled thence by land to Leyden. In this same letter is found the following injunction to his uncle, resident, be it remembered, in Ireland: "Dear Sir, keep all this a secret, or at least say it was for debt; for if it were once known at the University, I should hardly get a degree." Now had his uncle's tongue wagged ever so loudly, it could hardly have been heard by any university but that of Dublin, and that this University was actually in Goldsmith's mind is confirmed by another passage of the letter, which speaks of his expectation of being back in Kilmore in less than a year—i.e., two and a half years since he had left Ireland, and when he would be qualified both by study and lapse of time to present himself for examination for a medical degree.

At Leyden Goldsmith learnt the salutary lesson that medicine was more effectively taught in Edinburgh. "Physic," he writes, "is by no means taught so well as in Edinburgh; and in all Leyden there are but four British students owing to all necessaries being so extremely dear and the professors so very lazy (the chemical professor excepted), that we don't much care to come hither." Gaubius, we learn from the "Enquiry into the Present State of Polite Learning" (ch. ix), was the professor of chemistry, and with him Goldsmith was on terms of familiar intercourse. It is strange that beyond a mere mention in the

Percy memoranda, he should have nothing to say of the professor of anatomy, the great Albinus, whose anatomical plates were at that time unrivalled, and held their place among the best until comparatively recent times. One of the four British students in Leyden was Goldsmith's fellow-countryman, Dr. Ellis, a qualified medical man prosecuting post-graduate study. From him Percy learnt that Goldsmith was in chronic pecuniary straits, eking out existence by teaching his native language, and now and then resorting to the gaming table in the futile hope of re-establishing financial equilibrium, but there is no mention of any degree conferred on him at Leyden. Indeed, Prior prints in a footnote in his "Life of Goldsmith" (ed. 1837, i, p. 171) a letter of which the following is an excerpt: "Dr. Wenckebach, of Breda, has had the kindness to request of Professor Reinwardt to ascertain from the Album Academicum of the University of Leyden, whether Goldsmith was a student from 1754 to 1756, or whether any degree was conferred on him by that University; and the result of the inquiry is in each case in the negative."

There is no certain evidence of the precise duration of Goldsmith's stay in Leyden. The Percy memoranda fix it at "about a year," which seems to be correct, but in the same sentence "two years and a half" are assigned to his Edinburgh residence, which lasted but sixteen months. Anyone who is fully conversant with the available details of Goldsmith's life cannot fail to discern that it was at Leyden that the desire for travel came to prevail over the desire for medical study. How far this was due to the dullness of the professors, how far to his straitened circumstances, how far to extraneous causes, we have scant means of determining, though there is a very definite indication of what influence it was that directed his footsteps so soon as he had shaken the dust of Leyden off his feet. This was the example of the Danish writer, Baron de Holberg, who was much talked of at the time as a recently deceased celebrity. Writing of him in his "Enquiry into the Present State of Polite Learning," published in the spring of 1759, Goldsmith says:—

> "His ambition was not to be restrained, or his thirst of knowledge satisfied until he had seen the world. Without money, recommendations, or friends, he undertook to set out upon his travels, and to make the tour of Europe on foot. A good voice and a trifling skill in music were the only finances he had to support an undertaking so extensive; so he travelled by day, and at night sung at the doors of peasants' houses to get himself a lodging. In this manner, while yet very young, Holberg passed through France, Germany, and Holland."

Goldsmith might well have been penning the story of his own travels. Pending the publication of those letters, of the fortunate survival of which Sir Ernest Clarke has apprised us, we are dependent for further details of his travels chiefly upon some articles by his Irish friend, Cooke, published in the *European Magazine* in 1793. These, though of much interest, are of present importance only in so far as they show how impossible must medical study have been under the conditions. Such allusions, too, as can be found in Goldsmith's writings and letters all testify to the fact that his eyes were now set on the horizon of a wider world than the world of medicine; already his poem "The Traveller" was coming laboriously to birth.

Louvain seems to have been an early halting place. How long he stayed here we know not, though it was long enough for him to become acquainted with its professors and modes of study; we await evidence of his having even applied himself here to medical study in particular, and yet his first biographer and intimate friend, Glover, wrote in 1774, the year of Goldsmith's death, that he obtained the degree of Bachelor of Physic at Louvain, and no one appears to have questioned it at the time. Long ago Prior ascertained that the records of Louvain for the years 1754-56 had been lost or destroyed in the revolutionary wars, and that no appeal to them was possible, and inquiries instituted at Louvain in 1832 failed to elicit any information of Goldsmith whatever. The tradition of the Louvain degree seems to have passed current among Goldsmith's friends, for Dr. M'Donnell, who, as a lad, aged 18, had been befriended by him in London and acted for a short time in 1772 as his amanuensis, told Prior that such was his own recollection. Both Glover and M'Donnell were Irishmen, Glover indeed had actually studied medicine in Dublin, and each of them must have possessed abundant facilities for ascertaining the truth of Goldsmith having obtained a degree in Dublin. It is at least arguable that the knowledge that he had not done so may have engendered or strengthened their belief in the acquisition of a foreign degree. Moreover, the Percy memoranda, dictated by Goldsmith himself, make no mention of a degree acquired at any foreign university, coupled with an explicit assurance that he had taken a medical degree elsewhere. True, it is not explicitly stated that it was obtained at Dublin, but Goldsmith at the age of 20 had had no relations with any other university; moreover, on the occasion of the bestowal of the *ad eundem* M.B. degree by Oxford University, *Jackson's Oxford Journal* of February 18, 1769, describes him as Batchelor of Physic in the University of Dublin. In

face of this accumulated evidence the idea of a Louvain degree is quite untenable. The reappearance of the statement in so many of the biographies that appeared shortly after Goldsmith's death has perhaps earned credence for it, but when these are critically examined it is apparent, beyond all question, that the assertion has been derived by most of them from Glover's account, in the absence of any positive knowledge of their own.

Paris seems to have been the next important halting-place. Of medical study in Paris we know that he put in an appearance at the chemical lectures of Rouelle, for in his "Polite Learning" he speaks of the number of ladies in the audience. In various passages in his published works he gives us glimpses of his life in Paris and of the thoughts that were uppermost in his mind, but these were not of matters medical.

After quitting Paris Goldsmith seems to have accelerated his travels, going first to Germany, then to Switzerland, where he visited Geneva, Basle, Berne, and other places. Then he passed into Italy, making his first substantial stay at Padua, from which place he visited Milan, Mantua, Verona and Venice, penetrating as far south as Florence. At Padua the so-called Percy Memoir, attached to the four successive editions of Goldsmith's "Miscellaneous Works," first published in 1801, says that "he staid 6 months, and if ever he took any medical degree it was probably in this ancient school of medicine." Reference, however, to the work "De natione Anglica et Scota," published by Jo. Aloys Andrich, at Padua, in 1892, shows that the surmise was wrong. There is good reason to think that the stay was considerably less than six months, and as Goldsmith found time to visit from Padua several of the leading cities of northern Italy on foot, there can have been little opportunity for medical study, and we have, at present, no record that he did apply himself to medicine. But before finally dismissing the statement of the Percy Memoir, that it was at Padua that Goldsmith took a medical degree, if at all, we must know something of his relation to Percy, and something of the genesis of the Percy Memoir.

Percy, Bishop of Dromore, best known in connexion with his "Reliques of Ancient English Poetry," was first introduced to Goldsmith at his lodgings in Green Arbour Court, in March, 1759, and from that time till Goldsmith's death in 1774, cherished an intimate friendship with him. It was to Percy that Goldsmith, on April 28, 1773, dictated the materials for his own biography, which we have called the

Percy Memoranda. From this document we have already cited the passage, which can mean nothing, and can have meant nothing to Percy, but that Goldsmith had obtained his original medical degree at Dublin. That Percy received this statement with some reservation is apparent from one of the memoranda recently brought to light by Sir Ernest Clarke, in Percy's own handwriting, in which he says in connexion with the granting of the Oxford *ad eundem* degree in 1769: "N.B.—On this occasion Dr. Goldsmith was admitted *ad eundem gradum*, which he said was M.B." Turning now to the Percy Memoir we read in a footnote: "In February, 1769, Dr. Goldsmith made an excursion to Oxford with Dr. Johnson, and was admitted in that celebrated university *ad eundem gradum*, which he said was that of M.B." Percy was also of the party. Now the almost exact verbal identity of the concluding words of these two passages, and their very suggestive nature, show that each was written either by the same person or from the same notes. Whoever penned this latter passage must then have had before him Goldsmith's own statement, that he had taken his medical degree [at Dublin] at the age of about 20; yet the text to which the footnote is appended suggests that if ever he took a medical degree at all it was at Padua, when he was aged 26.

To identify the author of these passages we must turn for a moment to the composition of the Percy Memoir, as the champions of the Dublin medical degree do not permit it to be called in evidence, on the plea that it is the product of several hands. Samuel Johnson having neglected, as had been arranged, to write the biography of Goldsmith, Percy was constrained himself to undertake the task. Commencing about 1785, he spent several years in collecting materials from Goldsmith's relatives and friends. Among many who supplied him with information to supplement his own memoranda were Goldsmith's sister, Mrs. Hodson, and his sister-in-law, Henry Goldsmith's widow. So little progress was made that Percy handed his materials to an Irish clergyman, one Dr. Thomas Campbell, who produced an outline memoir by the autumn of 1791. The bishop then made copious marginal notes, and his chaplain, Mr. Boyd, redrafted the whole, incorporating the bishop's notes. Then followed a prolonged wrangle with the booksellers, resulting in the withdrawal of Percy from the post of editor, and the inevitable appointment of another editor, Mr. Rose. The Percy Correspondence does unquestionably suggest that Rose made interpolations into the text, but it also testifies that Percy accepted the memoir as his own work. Percy

cannot have believed Goldsmith's statement as to the Dublin medical degree, and if the suggestion of Padua came from him at all, it is probable that, out of loyalty to his dear friend, he did the best to suggest a *provenance* that was not inherently improbable, for there were Irish professors at Padua.

Having written Goldsmith down untruthful, it would seem desirable to state the grounds on which such an allegation is based. There is a remarkable unanimity among Goldsmith's biographers and friends as to the exuberance of statement that derived from his fervid Celtic imagination, and we need cite but a few illustrations out of many. Boswell, who appreciated his genius very ill, but his character very well, says of him: "His desire of imaginary consequence predominated over his attention to truth," and assuredly a medical degree would have given consequence to a struggling practitioner. Boswell cites actual examples of his untruthfulness; and here is other evidence from Mrs. Piozzi's Anecdotes: "Who will be my biographer, do you think?" Johnson asked. "Goldsmith, no doubt," cried Mrs. Thrale, "and he will do it the best among us." "The dog would write it best, to be sure," said Johnson, "but his particular malice towards me and general disregard of truth would make the book useless to all and injurious to my character." Percy, too, laughs in the same strain as Boswell, at his claiming relationship to Oliver Cromwell on account of his name, when he well knew it to be derived from his mother's father, Oliver Jones. It can hardly be contended that the assumption of a fictitious medical degree would have been incompatible with his habitual standard of veracity. Imagination sees things as they should be rather than as they are, and we love it none the less because of its peccant idealism. And if Goldsmith did sin, he sinned, at any rate, in a goodly company of spurious graduates, and in a contemporary professional atmosphere very tolerant of such obliquity.

Towards the end of 1755 Goldsmith left Padua and started on his journey home. After an arduous tramp through France on foot, finding lodging at convents, chiefly of the Irish nation, he landed penniless at Dover on February 1, 1756.

From the comfortable seclusion of one's writing table, it is no easy problem to predict the probable workings of a destitute wanderer's mind. Many of us will have landed at Dover empty, few penniless as well. The first impulse would likely be to make for London, working the way by any and every means coming to hand. This is what Goldsmith did, so says an acquaintance, seeking work in an apothecary's

shop in one of the towns through which he passed, but in vain, and arriving in London in from ten to fourteen days. Once in London, we may presume that he would seek the most profitable employment open to his attainments and qualifications. Now there is abundant evidence that he first sought employment as an usher, and failing this as "assistant" to various London apothecaries, but their doors were closed to a man who could not establish his identity, let alone his good character. And yet all the while the diploma of his alleged university degree must have been in his pocket! At last he came to temporary anchorage as journeyman to an apothecary, named Jacob, on Fish Street Hill, beside the Monument. The "journeyman" was the porter or man who delivered the medicines and made himself useful in the shop and laboratory; his duties lay with the broom and basket, not with the pestle and mortar, which were wielded by the apothecary and his "assistant." So far, then, there is no evidence that Goldsmith aspired to practise medicine.

While thus employed, his old Edinburgh friend, now Dr. Sleigh, found him, and from this meeting Goldsmith emerges, by the joint efforts of Jacob and Sleigh, as "physician in a humble way" in Bankside, Southwark. This is positively the first appearance of Goldsmith in the rôle of physician. One itches to know what passed at this meeting with Sleigh. Did his "desire of imaginary consequence" in the eyes of his successful fellow-student induce him to assume a fictitious degree? Did he produce his diploma and satisfy his friends of his claim? Did he adopt their advice to do what hundreds of others were doing in London, unlicensed medical practice?—and not a few of them under the shelter of a forged diploma of foreign graduation. We incline to the first of these alternatives as most in harmony with subsequent events. There is also a valuable piece of side evidence to be found in the biography attached to "The Poetical Works of Oliver Goldsmith. With a sketch of the Author's Life: Including Original Anecdotes Communicated by the Rev. John Evans, A.M., 1804." Evans says that his information was derived from one of the daughters of Dr. Milner, of Peckham, whose roof, a few months later, afforded constant shelter to Goldsmith on terms of intimate friendship. This memoir states that Goldsmith obtained a degree in medicine at Louvain, and also that he was an M.B. when he returned to London in 1756. We may take it, then, that these traditions were current in the Milner family; and from whom can they have obtained them but from Goldsmith himself, who will have spoken under the chastening influence of

the presence of young Milner, a doctor and former fellow-student of Goldsmith in Edinburgh?

His old Irish acquaintance and school-fellow, Beatty, has left it on record how Goldsmith bore himself in his practice:—

"He was in a suit of green and gold, miserably old and tarnished: his shirt and neck-cloth appeared to have been worn at least a fortnight; but he said he was practising physic, and doing very well."

Again "the desire of imaginary consequence" in face of poverty and want.

Prior gives another illustration, derived from Reynolds, of Goldsmith's desire at this juncture, at any rate, to *appear* a physician:"—

"In conformity to the prevailing garb of the day for physicians, Goldsmith, unable to obtain a new, had procured a second-hand velvet coat; but either from being deceived in the bargain or by subsequent accident, a considerable breach in the left breast was obliged to be repaired by the introduction of a new piece. This had not been so neatly done as not to be apparent to the close observation of his acquaintance and such persons as he visited in the capacity of medical attendant; willing, therefore, to conceal what is considered too obvious a symptom of poverty, he was accustomed to place his hat over the patch, and retain it there carefully during the visit; but this constant position becoming noticed, and the cause being soon known, occasioned no little merriment at his expense."

But, in spite of Goldsmith's brave words to Beatty, the truth was that as a physician he had not caught on, and was glad to find emancipation from an unprofitable slavery in an introduction by a printer's workman, one of his patients, to the printer Samuel Richardson, author of "Clarissa," from whom he now accepted a post of reader and corrector to the press. Here Dr. Farr, another Edinburgh fellow-student now in London, found him at work in his spare hours on a tragedy, about which he invited his opinion; but we soon find him installed as an usher in the school of Dr. Milner, at Peckham. Three months of this employment, hateful to Goldsmith in spite of the kindness of the Milners, drove him back to the precarious livelihood of a literary hack. A letter of December 27, 1757, to his brother-in-law, Hodson, in Ireland, defines his recent attempts to earn a livelihood. "By a very little practice as a physician, and a very little reputation as a poet, I make shift to live." This is important only in so far as it introduces Goldsmith to his own family in the character of physician.

Next year (1758) Goldsmith is back again as usher with Dr. Milner, who has promised to obtain a medical appointment in India for him.

Three months later the appointment of physician and surgeon to one of the factories on the coast of Coromandel is at his acceptance. He tells his brother-in-law that the East India Company have signed his warrant, and that he has paid £10 for it, while the expense of outfit, passage, and stores, will come to some £120 or £130 more. The salary, it is true, is only £100 a year, but the exclusive privilege of additional private practice is likely to bring in £1,000 a year besides. This Goldsmith holds out to his brother-in-law as his real inducement "to leave a place where I am every day gaining friends and esteem, and where I might enjoy all the conveniences of life"; and this at a time when he was quite unknown and face to face with starvation, with no friends but the small publishers who gave him casual employment as a literary drudge. The prospective expenses of the appointment were far more than Goldsmith could hope to earn by literature, and it is not difficult to detect the inwardness of his letter to his well-to-do, but prudent, brother-in-law. The books of the East India Company are silent on the subject of this appointment, but by November, 1758, it was cancelled. Goldsmith himself says that he was seduced by the assurance of literary occupation with Griffiths, the bookseller; but, even so, lack of funds may have been a contributory factor. Maybe Goldsmith was unable to produce evidence of qualification or professional competence.

Sir Ernest Clarke suggests that, in view of the proffered Coromandel appointment, Goldsmith applied to Dublin for the degree of M.B., and received it *in absentiâ*. Such an occurrence must have been known in the Milner family, by whom the appointment was secured, and yet their family tradition favoured the Louvain degree. His medical biographer, Sir James Prior, whose opinion justly carries great weight, thinks that Goldsmith was unwilling to cut himself adrift for so long, perhaps for life, from a literary career. Be that as it may, we find him on December 21, 1758, clad in a new suit, for which the bookseller Griffiths had advanced the money, presenting himself for examination at the Surgeons' Hall, erected six years previously in the Old Bailey. The result may still be seen in the register of the Royal College of Surgeons, from which the following extract is derived: "At a Court of Examiners held at the Theatre, 21st December, 1758. Present." Here the names of the examiners are omitted, but they were probably Messrs. Mark Hawkins, Fullagar, Nourse, Girle, Singleton, and Roul. A list of candidates follows, among whom we read: "James Bernard, mate to an hospital. Oliver Goldsmith, found not qualified for ditto." Prior

thinks that Goldsmith had determined to endure the briefer exile of an appointment to the naval or medical service, in lieu of the Indian appointment, but he does not tell us why a university graduate should be suing for the humble post of hospital mate, to enable him to undertake work for which such a medical degree would have already qualified him. We shall not be surprised to find that no mention of this examination is made in the statement dictated by Goldsmith to Percy, if, as we believe, he had adopted a fictitious degree, for such mention must needs have exposed the fiction.

From December 21, 1758, to March 31, 1763, the record of published literary work is so continuous as to put out of court the possibility of Goldsmith having devoted himself to studying for a medical degree, or indeed to medical practice except in vicarious fashion. The latter date brings us to the much quoted formal agreement, now in the British Museum, between James Dodsley and Oliver Goldsmith, signed by both of them, for the publication of a "Chronological History of the Lives of Eminent Persons of Great Britain and Ireland." In the body of this agreement, which presumably would have been drawn up by or for Dodsley, Goldsmith is alluded to as "Oliver Goldsmith, M.B.," but it is signed simply "Oliver Goldsmith." The signature is evidence that Goldsmith did not repudiate the medical degree assigned to him by Dodsley, but is not evidence that he then actually possessed the degree. Goldsmith had certainly enjoyed the titular prefix "Dr." before the date of this agreement, for example, in some of the advertisements of *The Citizen of the World*. The laity have always been lavish of the title on the flimsiest of pretexts: in the writer's native village the doctor's coachman was always known on the cricket field as "Dr." Its bestowal on Goldsmith, who had studied and even practised medicine, was inevitable, and is no guarantee of a medical diploma, still less of a medical degree.

On December 19, 1764, *The Traveller* was published bearing the name of "Oliver Goldsmith, M.B." From this time onward his name is seldom mentioned without the prefix "Dr."

In June, 1765, at the suggestion of Sir Joshua Reynolds, Goldsmith again and for the last time essayed the practice of medicine. Reynolds urged on him that a regular calling gave a man a social status. Garth, Cowley, Akenside, Smollett, and others had successfully combined literature and medicine. Prior, by the light of tailor Filby's ledger, displays Goldsmith in purple silk small-clothes, a handsome scarlet roquelaure buttoned close under the chin, a full professional wig, a

sword, and a gold-headed cane. In the ensuing six months Filby was requisitioned for no fewer than three similar suits; the right effect was to be produced at any cost. Next came a manservant, but still no practice; clearly Goldsmith's was a hopeless case. The only record of this new venture in practice that survives relates to his acquaintance, Mrs. Sidebotham. Her waiting-woman used to relate how he strutted into the sick-room of her mistress with his queer little figure stuck through with his sword, like a huge pin. One day a dispute arising between the apothecary and Goldsmith, the physician, on a matter of a dose, the lady wisely decided to follow the apothecary's advice, and Goldsmith left the house indignantly. "He would leave off prescribing for his friends," he said. "Do so, my dear Doctor," observed Beauclerc, the cultivated roué, "Whenever you undertake to kill let it be only your enemies." So ended this brief untimely venture in the rôle of physician, and four years of strenuous literary labour pass before we arrive at the next landmark of our investigation.

On February 14, 1769, Percy, Johnson, and Goldsmith went down together to Oxford, and there were guests of Chambers, then Vinerian Professor of Law, and five years later a judge in India. We have already cited the terms of the Percy memoir on this subject: "In February, 1769, Dr. Goldsmith made an excursion to Oxford with Dr. Johnson, and was admitted in that celebrated University *ad eundem gradum*, which he said was that of M.B." Turning to the Percy memoranda, we find the source from which the Percy memoir derived its statement:—

"Tuesday, 14 Feby. I went with Mr. Johnson and Dr. Goldsmith to Oxford."

"W. 15. We all dined in University College."

"Th. 16. We dined with Mr. Morthwaite at Queen's College,"

"Fri. 17. We dined with Tom Warton in Trin. Col."

"Sat. 18. We all returned to Town."

"N.B.—On this occasion Dr. Goldsmith was admitted *ad eundem gradum*, wch he said was M.B."[1]

[1] Even this Oxford degree, though its genuineness appears to be proven, is not entirely above suspicion. It is not entered in Percy's daily records under the appropriate day, and the event would seem to be at least as important as a dinner with Tom Warton; indeed, Percy considers it of such importance as to demand the prefix "N.B." Did Goldsmith's buffoonery prompt him to use *Jackson's Oxford Journal* as the medium of an elaborate hoax? There are other instances of jesting communications to the Press from his pen. We should understand then why the event is chronicled in Percy's daily record not on the day of its occurrence, but on the day of its appearance in the *Oxford Journal*.

In each record we find the suggestive words "which he said," whereas the admission to the degree is stated as an accomplished fact. Unfortunately there is a gap in the Register of Degrees conferred in Convocation just at this period, as indicated in the following letter of Dr. Bliss, quondam Registrar of Oxford University, to Sir James Prior:—

"Oxford, Feb. 24, 1834.

"Dear Sir,—I have now fully ascertained that no record of Goldsmith's admission *ad eundem* exists upon the registers of the university: but I have by no means ascertained that the Poet was not so admitted. On the contrary, I incline to believe that the Bishop of Dromore's impression was correct. It is a singular fact that there is a chasm in the Register of Convocation for 1769 from March 14 to March 18, which was the last day of Lent Term, and it is possible certainly that the admission of Goldsmith might have taken place in that interval. I told you I would mention the subject to the venerable President of Magdalen. I have done so, and he does not remember to have heard anything relative to Goldsmith's visit to Oxford."

That Goldsmith was actually admitted to this *ad eundem* degree seems to be placed beyond reasonable doubt by a recent find of the present Regius Professor of Medicine of Oxford in *Jackson's Oxford Journal*, for Saturday, February 18, 1769:—

"Yesterday Oliver Goldsmith, Esq., Batchelor of Physick in the University of Dublin, author of 'The Traveller, a Poem,' of the 'Present State of Polite Learning in Europe,' and of several other learned and ingenious Performances, was admitted in Congregation to the same Degree in this University."

Now it is pretty clear that Goldsmith can have produced no diploma of this degree, for the records of Dublin University are extant, and if ever recorded there, it would still be on record, which is not the case. Oxford University Congregation, therefore, must have acted on the strength of Goldsmith's statement. For the first and only time in his life Goldsmith is called upon to name the precise *provenance* of the supposed original degree, and he ties himself down to Dublin. On no other recorded occasion, when claiming the degree, as he did in his statement to Percy, does he give to it a local habitation and a name. So, by no other criterion than Goldsmith's own statements, we are asked to believe that Dublin conferred an M.B. degree on Goldsmith before he had commenced the study of medicine, after an unsatisfactory period of residence for his degree in Arts, and without making any record of the circumstance. And though numbers of his friends in London came from Dublin, and some had actually been his fellow-

students in that University, there was not one who, at his death, could assign his degree to its appropriate university, their general impression being that it had been gained at Louvain.

Goldsmith lived long enough to learn that a man's degrees are not the true measure of his greatness, and from assuming degrees that he had not we find him transfigured into flouting those that he had. Cooke, Evans, and other biographers say that towards the end of his life, and about the time of the successful production of "She Stoops to Conquer," he deliberately rejected the prefix "Dr." In the Prologue, written by Garrick, to this play, there are some very suggestive lines: no one who reads it can fail to see that Garrick is amusing himself, and prospectively his audience, by pulling Goldsmith's leg. The whole prologue should be read, and the following lines specially noted:—

> No *poisonous drugs* are mixed in what he gives;
> Should he succeed, you'll give him his degree;
> If not, within he will receive no fee!
> The college. *You* must his pretentions back,
> Pronounce him *regular*, or dub him quack.

In themselves these lines may be variously interpreted, but when all the evidence points in the direction of one interpretation, they must be allowed a cumulative effect.

In less than a year's time from this Goldsmith was destined to make his last reckoning with the medical science that he had successively courted and spurned. In March, 1774, he was hard at work on his poem "Retaliation," as well as on several other literary undertakings, that promised some measure of relief to his pecuniary embarrassments, when an old enemy assailed him. For some years, says Glover, he had suffered from an ailment, of which the leading symptom was strangury. The first unquestionable evidence we have of this particular disorder is in 1772, when he was attended by the famous Dr. James and obtained relief, seemingly, from the administration of his antimonial powder. There is little doubt, however, that the trouble was of longer duration than this, and Prior indicates that Goldsmith had permanently damaged his health by his dissipated life when a student at Edinburgh. Scattered allusions by Cradock and others show that, in the earlier attack of 1772, he obtained relief of the local symptom, but his general health remained affected, causing great depression of spirits as well as some irritability of temper: even when in company he was constrained to restrict his evening meal to boiled milk.

It is pretty evident that Goldsmith suffered from recurrent attacks

of cystitis, which, though temporarily relieved, were never completely cured, while by the backward invasion of his urinary tract his whole system suffered from chronic septic absorption. Of the special bacterial origin of this cystitis there is a good deal of suggestive evidence, as medical readers will readily anticipate.

Such, then, was Goldsmith's condition, when he returned from his Edgware lodging to London, in the middle of March, 1774, for the sake of more efficient treatment. There he saw a doctor and obtained relief from his strangury, but the symptoms of septic absorption became more intense, and he died with unmistakable evidence of profound affection of his kidneys.

On Friday, March 25, a low fever having supervened, Goldsmith took to his bed in his chambers in the Temple, and sent for the apothecary William Hawes at 11 o'clock at night. He complained of a violent pain extending all over the fore-part of his head; his tongue was moist; he had no cold shiverings nor pain in any other part, and the pulse-rate was about 90 per minute. He had already taken 2 oz. of ipecacuanha wine as an emetic, but was bent on taking James's powder as well, which he asked Hawes to send him. Hawes pleaded that his condition was more nervous than febrile, and that an opiate was the appropriate treatment to soothe his stomach after the emetic, while at the same time inducing sleep and relieving his headache. But Goldsmith was obdurate. In vain Hawes pleaded that the powder was likely to encourage instead of allaying his vomiting, and to produce diarrhœa with dangerous exhaustion. Hawes claims to have said, "Please, sir, to observe that, if you do take the fever powder, it is entirely without my approbation." But, in spite of this, he did actually send a supply of James's powders from his own apothecary's shop. He had, however, induced Goldsmith to call in Dr. Fordyce in his stead, and this physician repeated the advice that Hawes had already given. Nevertheless the patient administered to himself, with the assistance of his attendants, two or three powders, with the immediate consequences that Hawes had predicted. When Goldsmith found that the powders had not the desired effect, he exclaimed, "Damn that Hawes! I ordered him to send me James's powder, and he has sent me some other." A messenger was sent forthwith to the shop of the proprietor Newbery for the genuine powders, and these were administered by Goldsmith's servants. So strong was Goldsmith's impression that Hawes had sent a counterfeit powder, in spite of the fact that those procured from Newbery produced identical effects, that he took a dislike to him, and told

his servants to pay him his account and discharge him. A neighbouring apothecary, Maxwell, was actually called in, in his stead, but he seems to have declined attendance, as Goldsmith was already in good hands.

On the morning of Saturday, March 26, Hawes called, but, as Goldsmith's servant Eyles said that his master was dozing, he went away and returned in the evening. The servant then informed him that Goldsmith had vomited all day, and that profuse diarrhœa had persisted for eighteen hours, yet still his master had insisted on taking more powders. Hawes found him in a condition of profound exhaustion, with quick, thready pulse, and only able to say in a low voice that he wished he had followed his advice. The same Saturday evening Fordyce called on Hawes, and asked him to call in another physician, as Goldsmith rejected his advice. Accordingly on Sunday morning at 8 a.m., vomiting and diarrhœa having continued throughout the night, Dr. Turton associated himself with Fordyce in the care of the patient. These two visited him twice daily till his death.[1]

Throughout the ensuing week sleep deserted Goldsmith, and his growing weakness gave rise to grave anxiety: still he was conscious, and spasmodic efforts at cheerfulness seemed to encourage hope. Turton, noticing the state of his pulse, said to him, "Your pulse is in greater disorder than it should be from the degree of fever which you have. Is your mind at ease?" "No, it is not" were the last words to pass his lips. On Sunday night he fell asleep, but at 4 o'clock on Monday morning the apothecary, Maxwell, was summoned in great haste, and on his arrival found him in strong convulsions, which continued without intermission until his death at 4.45 a.m.

Three days after his death Horace Walpole wrote to Mason that he had died of a purple fever. While accepting with caution lay evidence at secondhand, it may be said that a purpuric eruption would be quite compatible with the other features of the illness.

The news of Goldsmith's death came as a profound shock to his friends: Johnson, Burke, Reynolds and the rest bowed their heads in sorrow. But most moving tribute of all, a company of poor women, the flotsam and jetsam of this great soulless city, gathered before the death-chamber door to shed a parting tear for their dead friend. "Let not his frailties be remembered, he was a great man." That was Johnson's requiem, and let that be ours.

[1] It is impossible to piece together an unimpeachable description of Goldsmith's last illness in detail. Hawes's account is very full of detail, but must be accepted with considerable caution. The affidavits appended to it read like fair specimens of hard lying.

The PRESIDENT (Dr. Norman Moore) said that Oliver Goldsmith was a great and wonderful man of letters. He had conferred enormous and lasting benefit on English literature. His own perfect style had improved the English language in a variety of ways. He lived in the very best intellectual society of his time, in friendly relationship with the greatest group of literary men that has ever existed in England. Men of the highest honour and truthfulness, who scorned all forms of deception, profoundly admired him. Burke's affection for him alone would show that Goldsmith was not an untruthful man. Johnson, who scorned deceit and inaccuracy, respected and loved him. Goldsmith was an Irishman, and perhaps for this reason has often been imperfectly understood by the English. Yet no one acquainted with the Irish could fail to understand his character. His chance statements, seemingly inaccurate, were capable of explanation. For instance, the story of his claim to descent from Oliver Cromwell, although it was known that he was called after his relative Oliver Jones, might have arisen from his mentioning one Oliver Jones, the Parliamentary General, who won a great victory in a part of the country known to Goldsmith. Goldsmith's honour and integrity were notified by the company he kept, that of Burke and Johnson. He (Dr. Moore) felt that the right view to take was that, although we have failed to discover where and when he took his degree, it is impossible to believe him guilty of fraud in the matter. Eighteenth century university records were sometimes loosely kept. In Trinity College, Dublin, they were generally accurate, but it would be too much to assume that no omissions existed. Might not records have been omitted at Louvain and Padua, as at Oxford? He regarded Goldsmith as an upright man, and a benefactor of literature and mankind, and should continue to believe in his veracity.

Section of the History of Medicine.

December 16, 1914.

Dr. Norman Moore, President of the Section, in the Chair.

400th Anniversary of Vesalius.

The President (Dr. Norman Moore) desired that it should be recorded that this Society had not ignored so important an event as the approaching 400th anniversary of the death of Vesalius. The war had rendered impossible the "Vesalius Celebration" which should have been held this month jointly at Brussels and London. Yet he (Dr. Moore) wished it recorded that the Royal Society of Medicine did remember Vesalius with the utmost respect and veneration. Some knew him through the wonderful plates in his anatomy, others from the fact that the second founder of a Cambridge college had actually spoken to him at Padua. His own interest in Vesalius dated from the time when, as undergraduate at Cambridge, he listened to Professor Kingsley's lectures on Vesalius, Rondeletius and Montpellier. Our memory of Vesalius, who did so much to place the science of anatomy on a modern basis, should never be allowed to fade.

A Westmorland Medical Superstition.

By G. C. Peachey.

The family of Le Fleming have been seated at Rydal in Westmorland since the early years of the fifteenth century, when they acquired that estate by marriage. In the time of the Commonwealth it was in the possession of the much-honoured Daniel Fleming, of Beckermet and Rydal—for so he described himself upon his book-plate—and it is necessary for our purpose that we should form some estimate of what manner

of man he was. Royalist by tradition and sympathy, at the Restoration he filled the office of Sheriff of Cumberland, becoming later M.P. for Cockermouth. By competent authority he was described as the most accomplished Justice of the Peace of the northern counties; he was the author of a Description of Westmorland—one manuscript of which is now in the Bodleian Library, which was utilised by Burn in his history of Cumberland and Westmorland; and the fact that his antiquarian knowledge was requisitioned by such men as Sir William Dugdale, Blome, the editor of "Guillim," and other historians, supports the above encomium upon his learning.

That Daniel Fleming was not wanting in a sense of his own or his family's importance is evidenced by the inscription on his book-plate, claiming that the Manor of Beckermet had been held by his ancestors ever since a few years after the Conquest; and by his written request in 1675 that, in consideration of his services, he should be made a knight or baronet, "ten of his ancestors having had that honour." On May 15, 1681, he was knighted at Windsor by Charles II, as a reward for his loyalty and devotion to the dynasty.

In 1655 Daniel Fleming married, and on July 26, in the following year, a son and heir was born to him. When this boy, William, had just completed his second year he sustained an injury, the treatment of which forms the subject of the present inquiry. The accident happened on August 10, 1658, on which day an entry was made in his father's diary :—

> "Given unto George Browne of Troutbeck, a bone-setter, when Will was hurt, 2/6."

The village of Troutbeck is about five miles from Rydal.

Apparently the opinion was not considered satisfactory, for the very next day further advice of the same sort was obtained.

> "11 August, 1658: Given unto William Story of Sedgewicke, near Sighser, bone-setter, for lookeing at Will's thigh, 7/6."

Sedgwick is a still smaller village, south of Kendal and about twenty miles from Rydal.

The seat of the injury is here definitely located, though in Kimber's Baronetage it is said that, "by an unhappy fall, he so sprained or dislocated some bone in one of his ankles, that notwithstanding all the assistance that could be had from the physicians and surgeons of the greatest repute at that time, either in the country, at London, or at the Bath, he continued lame after so long as he lived."

For three months nothing further appears to have been done until we find against the date November 12, 1658:—

"Given unto John Rawling, a bone-setter, for Will, 10/-."

The talent of the immediate neighbourhood having been tried without success, Daniel Fleming had gone farther afield to Cockermouth, an important borough and market-town in the adjoining county of Cumberland, and about thirty miles from Rydal.

The skill of the Fell-side bone-setters has long been a settled tradition, the secrets of the craft being handed down to successive generations of certain families, conspicuous amongst whom in later times have been the Huttons and Atkinsons, old Westmorland yeoman stock. Representatives of these eventually found their way to the southern metropolis, and, though opposed by professional prejudice, they forced themselves, first into public notice, and afterwards into no small measure of popular esteem. The chief exponents of the art have demonstrated their possession of peculiar talents, arising in part from hereditary instinct, and in other part from natural adroitness improved by anatomical study; many of them being characterised by great physical strength and large hands, which latter feature has been common to most of the best-known manipulative healers, from Valentine Greatrakes onwards.

When it is remembered that wrestling was then, as now, the popular sport of the district in which he lived, and that the skill of the local bone-setters was constantly being displayed in the cure of those injured in the ring, one can scarcely be surprised that the mind of the much-honoured Daniel, lover as he was of all that related to the past, should have been tinctured with a belief, not entirely free from superstition, in the traditions imputing to them mysterious powers of healing.

And here it must be remarked that the diary from which these extracts are taken was evidently very carefully kept, and there is reason to suppose that every item of medical expenditure would have been scrupulously entered. There are several entries of payments at various times to the local apothecary, Mr. Kempe, probably resident at Grasmere or Ambleside, but none of them occurs during the year 1658, so that it may be assumed that until the following March—eight months after the accident—the opinion of no regular medical practitioner had been obtained. Then we find the entry:—

"10 March, 1659: Given unto Docter Dykes for comeing and laying plasters to Will, 10/-."

This was Dr. Thomas Dykes, of Cockermouth, probably the nearest local physician, who, in the following month, appears to have given this written opinion:—

> "12 April, 1659: I doubt nott butt he will overcome his Distemper; he cann both contractt itt [presumably the thigh] upwards and also extend itt Downwards: and wth the helpe of his maid walke a turne or tow very prettylie over the chamber: and this for six weekes tyme is as much as cann be expectted."

In view of this favourable prognosis the treatment was probably continued for a while, but July 22, 1659, we read:—

> "Paid Dr. Dykes—besides £5 paid him 10 June—for his plaisters and paines contributed towards ye cure of Will, the sum of £5."

It looks as if the attendance terminated here, for no record appears of further payments to Dr. Dykes; and it may be that sufficient improvement had been manifested to support the hope that time and rest would effect a cure. But the months passed by, and little Will Fleming, now three years old, was still a cripple.

Imagine an anxious parent, his first-born son afflicted with a malady, upon the exact nature of which the opinions of those who had been consulted almost certainly disagreed, and whose treatment had hitherto proved unavailing. Short of the Metropolis, no first-class advice was obtainable, and London, at that particular time, was not the place in which Daniel, with his political views, could expect to spend a happy day. Casting over in his mind what he could do for his boy; asking, or we may be sure at least receiving, advice from his friends and relations, he shot his arrow at a venture, and resorted to one of the most curious superstitious remedies which has ever been chronicled, even in the annals of medicine. Says the diary in laconic phraseology without explanation or comment:—

> "25 January, 1660: Given unto James Richman (ye Tench-Docter), for comeing unto Rydall to see Will, 10/-: More to his two Clerkes, 4/-: More to Christ. Parker (y^{e} Glasier), who gott him to come, and who came along with him, 5/-."

Who James Richman was; why he was called a tench-doctor; where he learned the secret of his craft: what its precise nature was; and for what diseases he practised it; we are left to discover. The bare facts of his name and description; his fee and that of his two

clerks; and that he came by persuasion of the glazier, are all that is told.

So far as can be ascertained, no other mention occurs in literature of any such calling as a tench-doctor. Professor Wright in his English Dialect Dictionary gives nothing relating to the tench or its folk-lore, and an inquiry I addressed some years ago to Sir James Murray, elicited that neither had any information to impart. Old Isaac Walton says that the tench is the physician of fishes, of the pike especially; that the pike being sick or hurt is cured by the touch of the tench; and that the tyrant pike will not be a wolf to his physician, but forbears to devour him, be he never so hungry.[1] Caius,[2] speaking of tench, calls them good plasters but bad nourishment: "for being laied to the soles of the feet they often draw away the ague"; to which Nares quaintly adds, by way of annotation, that they are *now* much more frequently put *into* the stomach than applied externally. Goodall, in his "Account of Empirics," tells that, in 1615, William Blanke, an illiterate fellow, without learning, was fined and imprisoned by the President of the College of Physicians for having administered medicines to one who, he said, had "a convulsion of the stomach, with infection (? infarction) of the liver, and raised his lungs into his throat, which he brought down by applying a tench to his back."

Although these are the only forthcoming references to the use of the tench as an external application in English medicine, they are sufficient to show that the practice was known in this country, although probably not extensively adopted. We have to go to foreign writers to discover the *motif* and technique of the treatment. Rondeletius, a contemporary of Caius, records having seen a great cure done at Rome by applying a tench to the feet of a very sick man. Crollius[3] tells that the tench applied to the abdomen, and retained there until it dies, is useful in jaundice, especially in the jaundice of plague. Castellionæus[4] advises the same remedy for that complaint, adding that a yellow humour is thereby drawn away from the umbilicus. Aldrovandus[5] suggests that the fish should be bound alive to the patient's liver or umbilicus in jaundice; and that the treatment should be continued, with relays of

[1] Walton, "Compleat History of Fishes," pt. i, cap. 11.

[2] Caius, "History of Animals," p. 227.

[3] O. Crollius, "De Signaturis," p. 34.

[4] P. M. Castellionæus, "De Admirand. Natural.," syntag. 5.

[5] U. Aldrovandus, "De Piscibus," lib. 5, cap. 45.

the living fish, for three days. He further relates that others use them slit up the back lengthwise, as an application to the soles and palms in high fever. All these authorities were living about the end of the sixteenth century.

In 1637, John Baptist Sitonus, a physician of Milan, devoted a moiety of one of his tracts to the question—whether the application of the tench to the abdomen of patients affected with jaundice is curative of that disorder?[1] After examining the opinions of the above authors, he arrived at an affirmative conclusion, on much the same grounds as those used by Sir Kenelm Digby before the physicians of Montpelier (*c.* 1657) in explanation of the so-called natural methods (a sort of correlation of forces, described by him as emanations of light, heat and motion), by which he claimed that his Powder of Sympathy effected its power of healing. Sitonus affirms that "the tench applied alive to the umbilicus cures jaundice by reason of sympathy and signature, for the tench carries the signature for jaundice in its colour." The effect of the sympathy and signature (itself an astrological emanation) was manifested by a process of derivation, the fever and ague being drawn out by application of the fish to the soles and palms, and the yellow humour being brought away by application to the umbilicus.

The visible manifestation of the death of the animal, which appears to have been stipulated for in each instance, would, in the minds of the superstitious, be sure evidence of the transference of the disease, which anxious credulity would speedily exaggerate into proof of cure; and that such cures by transference *were* (and likely enough still are) believed in by the Cumberland folk, is shown by the following instance quoted by Black in his "Folk Medicine"[2]:—

> "An old fisherman, formerly well known at the Forge, Keswick, once caught a fish which he put into the mouth of a child suffering from whooping-cough. He then replaced the fish in the river: and affirmed that, after being placed in the mouth of the child and returned to the river, the fish gave the complaint to the rest of its kind, as was evident from the fact that they came to the top to cough."

It must, unfortunately, remain an open question whether Richman's treatment was undertaken for the cure of the original injury, or whether for an intercurrent attack of jaundice. The testimony of the medical authors quoted entirely favours the latter supposition, inasmuch

[1] J. B. Sitonus, "Misc. Med. Curiosa," 1677, 2nd ed., tract 27.

[2] W. G. Black, "Folk Medicine," 1883, p. 36.

as they one and all make reference to jaundice, ague, or fever, without mentioning joint or bone lesions. On the other hand, there is no record in the diary of any attendance of the local apothecary, and it would, indeed, have been remarkable if such a complicated and out-of-the-way method of treatment had been adopted for an attack of jaundice in the first instance and without trial of the ordinary measures.

Until the discovery of the accepted use of the tench as a therapeutical agent, it had seemed, bearing in mind the traditional rôle assigned to it as the physician of fishes, that healing by the tench might, by metonymy, have acquired the meaning of healing by touch, and a tench-doctor the meaning of a touch-doctor; in much the same way that tact and taste, attributes taken from the body, have been applied metaphorically to the mind. This idea was confirmed on finding that the late Dr. Payne had been consulted upon the matter,[1] and had given it as his opinion that Richman was one who treated diseases by rubbing and stroking, after the manner of Valentine Greatrakes, seventh sons, and others; though it is admitted that the use of the fish itself as the remedial agent might be the explanation. In the light of further investigation, however, there can be little doubt that Richman's treatment consisted of the actual application of the living fish to the patient's body; while the presence of his two clerks suggests their employment in slitting up the unfortunate animals, while still living, and in applying relays, as ordained by Aldrovandus. And the opinion that Richman's method was not by stroking or rubbing is strengthened by an incident chronicled by Daniel Fleming in 1669. Against the date, October 2, of that year, appears an entry in the diary:—

> "Given unto my brother Roger towards his charges in goeing unto London to get the King's Touch for the Evill, £10."

The mere fact of his providing a portion of his brother's expenses for that purpose argues a substantive belief in its efficacy; added to which, Daniel Fleming, as a devoted Royalist, doubtless gave credence to the possession of the gift of healing by the Most Sacred Hand of His Most Divine Majesty, Charles II, which royal hand he hoped to kiss (as he eventually did) on receiving knighthood. Had he received any encouragement to suppose that his boy's ailment was of a scrofulous nature there can be little doubt that, instead of going to London to

[1] Magrath, "The Flemings in Oxford," i, p. 559.

obtain surgical advice (as we shall see he did two years later—that is in 1662), his son would have obtained the monarch's touch during that visit; or, failing that, would have joined the throng at the Banqueting House at Whitehall, in 1669, with his uncle Roger.

Yet in the absence of any further evidence it may fairly be assumed that on neither of these occasions was King Charles's power of healing put upon its trial by Will Fleming; and it is not conceivable, even had Richman been a touch-doctor, that under any circumstances Daniel Fleming, with his Royalist proclivities, would have preferred Richman's reputed power of healing by touch to that of his sovereign. Who Richman was, and where he learned the secret of his craft, are still unanswered questions. My inquiries, primarily directed to the discovery of his parentage, residence, and whether he was a seventh son, have in each respect been futile. The fact that he is described as plain James Richman is something against the latter proposition, since it was no uncommon custom, particularly in the North, to find the seventh son christened either Septimus or Doctor, in accordance with the tradition which imputed to the seventh son born in succession, no daughters intervening, innate powers of healing. Failing to ascertain his parentage, and the occupation of his father, I can only suggest, remembering that Isaac Walton published his "Compleat History of Fishes" in 1653, just six years before we hear of Richman, that the latter may have come across this work, and, reading therein of the cures mentioned by Rondeletius and Caius as effected by the agency of the tench, may, as the result of further study, have decided to turn his knowledge to substantive account. The secret of his craft, if secret there were, appears, however, to have died with him; in no instance do we find later reference to such a calling.

Before finally leaving Richman and his tench a passing note may be made of the fees which have been quoted. At that period the due of a graduate in physic was about 10s.; licensed physicians were entitled to 6s. 8d., but generally asked 10s.; and a surgeon was paid 1s. a mile, whether his journey was near or far, with 3s. 4d. for setting a fracture or reducing a dislocation. In comparison, we find that the three bone-setters were paid sums varying, according to distance, from fourpence to sixpence a mile, while the Cockermouth bone-setter, the Cockermouth physician, and the tench-doctor received 10s. each; a strange position for a physician to be seated on the same bench between two irregular practitioners.

Little more remains to be told. In July, 1661, the boy, then five

years old, was taken to St. Mungo's Well, at Copgrove, in Yorkshire, one of the holy wells of England, dedicated to St. Mongah (Kentigern), to whose saintly virtues its healing powers were attributed by the superstitious. Cold bathing was just again becoming popular after its temporary abandonment in consequence of the change in religious opinion, which, in the first place, had condemned the use of wells dedicated to the saints, and in the second place had declared baptismal immersion (and inferentially, according to Floyer, cold bathing) to be not only immodest but also dangerous.

Early in April, 1662, we find Daniel Fleming setting out for London with his wife and child. After breaking the journey at York, they reached town towards the end of the month, and took lodgings "near the 'Seven Stars,' over against Somerset House Yard in the Strand." There they remained about two months, during which time the child appears to have been under the treatment of William Molins, a member of a well-known family of surgeons, and himself a man of repute in his profession. The last item of the expenditure in London, reads:—

> "June 29, Paid unto Mr. Will Moolin, the chirurgeon, the sum of £10—in full, if Will prove not cured, and if cured, then £20 more."

Such methods of bargaining were countenanced by even the most eminent medical men at that time, while payment in kind was no uncommon custom; but, since no further mention of his name occurs in the diary, it may be assumed that Molins never claimed the larger sum, while it is certain that, in spite of the two months' treatment, Will was not cured.

Nearly six years later (May, 1667), we hear of the boy on his way to Bath, breaking the southward journey at Oxford in order to consult Dr. Thomas Willis, one of the most famous physicians of his day, by whom he was passed on to the care of Dr. John Maplet, who divided his practice between Bath in the summer and Bristol (then a popular spa) in the winter.

At the age of 12 he was well enough to join his brothers at school in Kendal; but he had to use crutches, and for several years subsequently he visited Cartmel and took the waters there. He left school in 1676, and does not appear to have gone to the University, probably in consequence of his affliction.

Until 1693 nothing more is recorded of his ailment; he had attained to manhood ere we hear of it again. On April 12, 1693, Sir Daniel, knighted as we have seen, writes to him:—

"I hope you will perfect the cure of your leg at the Bath, and also the cure of your itch of rambling and talking and scribbling, imprudently and undutifully." William had become a man of fashion, and had evidently displeased his much-honoured parent. He had suffered much from that fall—perhaps the fault of some careless maidservant at Rydal; and we fancy that he was making the best of life in the western metropolis of fashion. Two years later he was elected M.P. for Westmorland, and upon his father's death in 1701 retired to the country to look after the estate to which he had thereby succeeded.

On October 4, 1705, he was created a baronet, with remainder, for want of issue male of his body (he being still a bachelor), to the issue male of his father; and the same month Lord Godolphin sent him a discharge for £1,095—the sum usually paid to the Exchequer—in respect of his baronetcy. He married in 1723 at the age of 67, became the father of three daughters, and died in 1736 without male issue, whereupon the title and estates passed to his next surviving brother, George Fleming, Bishop of Carlisle.

BIBLIOGRAPHY.

"The MSS. of S. H. Le Fleming, of Rydal" (Hist. MSS. Com.), 1890, *passim*.
Magrath. "The Flemings in Oxford," 1904, &c., *passim*.

The Apothecary in England from the Thirteenth to the Close of the Sixteenth Century.

By C. J. S. Thompson.

Little is known of the apothecary in England from the thirteenth to the close of the sixteenth century, and the details recorded by the medical historians are very few. Freind, in his "History of Physicke," written in 1725, mentions two—namely, Pierre de Montpellier, who was apothecary to Edward III in 1360, and J. Falcand de Luca in 1357, who he describes as the first apothecary to sell medicines in England.

The notes I wish to place before you are the result of a careful search through some of the early existing State Records. These include the Pipe Rolls, Privy Seal Warrants, Patent and Close Rolls, and

Records of the Great Wardrobe. They have yielded, I think, some interesting and hitherto unpublished details concerning the apothecary in England at this interesting period.

First I should like to say a word or two respecting the origin of the apothecary as a separate practitioner in this country. From evidence gathered from these and other records it would appear that the apothecary first commenced to practise his art, as distinct from the practice of medicine carried on by the physician, about the middle of the twelfth century. He was apparently an offshoot from the pepperer. A Guild of Pepperers as a body is mentioned in the Pipe Rolls of 1179-80. They were a company of traders or merchants, who imported medicinal and other kinds of spices, together with drugs, from the shores of the Red Sea and various Eastern ports. In 1221 there is a record of one Andrew Bokerell, a pepperer who was also Keeper of the King's Exchange. His duty in connexion with the latter office was to receive old stamps or coining irons and deliver new ones to all the mints in England.

Some time after the formation of the Guild they amalgamated with another body called the Spicers, who traded in similar wares, but they ultimately fell into difficulties, and in 1345 a new fraternity of Pepperers was founded. In London they were known as the Pepperers or Easterlings of Sopers Lane, and the Spicers of the Ward of Chepe, from the districts in which they carried on their business, where they had stalls or shops for the sale of perfumes, spices and drugs. They paid a toll to the King in kind—namely, a certain quantity of pepper.

A considerable number of the Spicers were Italians, while the Easterlings were chiefly Germans from the Baltic coast and the Hanse towns, who brought goods from the East and placed them on the English market through the Pepperers and Spicers, who thus became their dispensers or distributors. The latter were a progressive body and introduced improvements in the coinage from Constantinople. It was from them the new *sterling*[1] money was named, which was first made in England in 1180 to take the place of the then debased currency. St. Anthony was their patron saint, and up to the year 1373 they bore the title of the "Fraternity of St. Anthony."

[1] This ingenious etymology has many learned supporters, but *esterlin* is used as the name of a coin in the *Roman de Rou* (? 1170). It is suggested that the first issue of the coin was stamped with a star. Cf. the Scottish "bawbee"—the first halfpenny issued, bearing the effigy of the *baby* James VI.—Ed.

According to their ordinances in 1376, "no one of other mistery shall be admitted into the company without the common consent." A spirit of jealousy, however, grew up between the two branches, and as years went on the breach became wider, the brotherly love and unity of the promoters evidently became a dead letter, and the title of the "Fraternity of St. Anthony" was dropped and that of the "Company of Grocers" substituted, which in 1428 was granted its first charter by Henry VI.

There is, therefore, little doubt that the pepperers, spicers and apothecaries were originally branches of the same trade, the latter dealing more in drugs for medicinal use, and the former in the spices and condiments employed for domestic purposes. This is corroborated by the particulars of their stocks which are still on record. In addition to drugs the apothecaries kept the rarer kinds of spices, exported from the East through Italy, aromatic gums, wax candles, pepper, and sugar. These came mainly from the East and were regarded as luxuries and not daily necessaries.

The earliest authentic record we have been able to find in which an apothecary is mentioned by name is in the year 1290, in the reign of Edward I. From an entry in the Great Wardrobe Book we learn that while Queen Eleanor was travelling through Lincolnshire to meet the King, she was seized with an autumnal fever at Hardeby, near Grantham. The illness was a lingering one and it was not until she was in a precarious position that the King was informed of the fact. Henry Montpellier, one of her apothecaries, was paid 13s. 4d. for syrup and other medicines bought at Lincoln on October 28, 1290, for the Queen at Hardeby. From this entry it must be concluded that at this period an apothecary held a Court appointment which was distinct from that of the physician, for it is further stated that Master Leopardo, the Household Physician, was in attendance on her in addition to a leech in the service of the King of Arragon. Henry Montpellier was probably a Frenchman who had studied his art at the ancient centre of learning of that name, and had come to England from France in the service of the Queen.

In the year 1292 there is mention of an apothecary's shop in York where Master Otto, of Germany, a physician of repute, had his medicines compounded.

In our cities in the thirteenth century the apothecaries appear to have congregated in certain localities called the Spiceries or Apothecaria, and in the markets their stalls were often placed together. The earliest

record we have been able to find concerning this custom is in a deed dated 1297 among the Records of the City of Oxford. It is there stated that on the site of the present market there was formerly a district called the "Spicery," which was allotted to the apothecaries and spicers to carry on their business. In this document the stalls are described as being in the parish of St. Mary, but in a later deed it is stated the Spicery was in the parish of All Saints. The earliest use of the word "Apothecaria" applied to this district is in a deed dated March 22, 1315-16, which is included in Shadwell's "Calendar of Oriel Documents." It is a "Release by John Sencer of Oxford to John, sone of William Espicer of Oxford, of all his lands, tenements and rents which he has by extent under a writ of elegit, viz., a seizure of property of a party legally declared unable to pay a debt, a messuage in the parish of St. Mary the Virgin, three shops adjoining it, two shops in the Apothecaria in All Saint's parish."

According to another document, dated 1332, one part of this locality was called "Apothecaries Rew or ye place where ye apothecaries shopps were in All Saints parish. This Profession is very antient in Oxon, and seemeth to have been from ye first under ye jurisdiction of ye Chancellor. At what time it was planted here there is doubt."

There are several other allusions to be found concerning shops and stalls in the Apothecaria, or Ipoticaria as it is sometimes called, in deeds dated 1335 and 1341, but we will quote but one other, which is somewhat quaint and interesting, dated 1341, in the fifteenth year of the reign of Edward III, in the King's Court: "William the Taper-maker delivered to John of Denton his Ypotecary-stall with all its utensils, and 20 lbs. of wax and spices existing in the stall to keep a merchandise for him, and to give account of the same at the four seasons of the year. He now complains he cannot get the account from John of Denton."

Further mention of an apothecary by name is to be found in the Dering Papers, in which there is an entry that in 1308 at Pluckley, Kent, a grant of a right-of-way was made by one John de Sellinge, Eppecer of London, to John, son of John Malmayns, of Waldershall; also of a quit claim of the same, wherein Sellinge is termed "apothecarius." In a later deed, dated 1317, the same individual is termed a "citizen and apothecary."

In the Wardrobe Rolls of 1313, as already noted by Mr. D'Arcy Power, there is record of one Odin, the Spicer, who was an official at the Court of Edward II, and received 7½d. a day as apothecary to the Queen.

In the year 1329 we find mention of an apothecary called "John" in the Scottish Exchequer Rolls (in connexion with the death of the famous Robert the Bruce) to whom payments were made for materials for embalming the King's body.

In a State Record, dated September 3, 1330, in the third year of the reign of Edward III, there is an interesting entry concerning an ordinance of the Treasurers and Barons of the Exchequer directing the payment of 5s. 6d. to Master Peter de Montpellers (Montpellier), the King's apothecary, who was then staying with Gilbert Talebot and Master Pancius de Controne, the King's physician, at the Manor of Hoxne, who were then sick, and to two grooms and one horse of Peter's, their necessaries during their stay.

A like order of an allowance of £7 2s. 8½d. to be paid to Master Pancius de Controne, who was then ill, to John Luca (apothecary), then staying with Pancius, and to two men and two horses of the said John their necessaries during the time of their stay.

The Master Peter de Montpellers may have been related to the apothecary who flourished thirty years later, referred to by Freind, and it is also possible that the John Luca, apothecary, is the same John Falcand de Luca who is mentioned by Good as being an apothecary in London twenty-seven years later.

Rymer, who was appointed by William III to compile his records from the official State Papers in 1704, states that a pension of sixpence a day was granted to Coursus de Gangeland, an apothecary of London, in 1345, in recognition of his services to the King during his illness in Scotland. Rymer's records are generally regarded as accurate, though we have been unable to find any confirmation of the statement in the State Records of the time.

Chaucer's well-known allusion to the apothecary in the "Prologue to the Canterbury Tales," written about 1387, proves that the practice of his art was distinct from that of the physician at this period.

An entry of medical interest appears in the Great Wardrobe Papers dated February 27, 1428, in the reign of Henry VI; by a writ of Privy Seal the King's physicians are paid £40 a year each, also a livery (uniform) with fur and lining as other Royal physicians have had yearly at the Great Wardrobe by the hands of the keeper thereof. This is especially interesting, as later on we are given some authentic details of the costume or robes of the physicians and apothecaries attached to the Royal Household which probably date back to a period earlier than the reign of Henry VI.

In 1428 the Company of Grocers built their hall upon the site of Lord Fitzwalter's mansion in Prince's Street, and obtained their first charter of incorporation from Henry VI at the same period, without any condition as to trade, but simply as a religious or social guild, with powers "to possess lands, tenements and rents, and other possessions whatsoever."

In an entry in the Privy Seal Records in 1454 mention is made of the Court dress of the King's physician, which is described as being a robe of green cloth with a miniver cap.

In the same year there is an interesting record worthy of mention concerning the illness of King Henry VI. When he was taken ill a medical commission of five physicians and surgeons was appointed by the Duke of York and his Council on April 6, 1454, to attend on the person of the King and to watch over his health. The commission empowered "those beloved masters, John Arundel, John Faceby and William Hacliff, physicians, and Robert Warreyn and William Marshall, surgeons, to administer to the King at their discretion electuaries, potions and syrups, confections, and laxative medicines in any form that may be thought best; baths, fomentations, embrocations, unctions, plasters, shavings of the head, scarifications, and a variety of other inflictions in the way of medical treatment." This enumeration of the preparations employed throws an interesting light on the medical treatment of the period. John Faceby was the favourite physician of Henry VI, whom he attended all his life. On the occasion of his marriage with Queen Margaret of Anjou the King granted him a pension of £100 as a reward for his faithful services.

That the devoted services of the medical practitioner in those days did not go unrewarded may be judged from the following entries:—

In 1455 it is recorded that a grant for life was made to the King's serjeant, William Godfrey, who combined the office of yeoman of the chamber with that of apothecary to the King's person and porter of Exeter Castle, co. Devon, to hold himself or by deputy, with the usual wages, fees and profits.

In the following year there is a record of a like grant being made to the said William Godfrey during his life as yeoman of the chamber and King's apothecary and "as garbler of all spices and drugs, and all merchandise which ought to be garbled in London, Southampton and Sandwich, to hold himself or by deputy; taking such fees as were wont to be taken in London heretofore on account of the office; so that no merchant expose for sale such spices, drugs and merchandise

in the said city and towns until the same are garbled, under pain of forfeiture thereof."

The office of garbler in relation to the apothecary is interesting, and originated as follows: Dissensions arose between the apothecaries, the spicers and grocers owing probably to the competition in the sale of similar wares. About the middle of the fourteenth century the former accused the latter of adulterating their goods, while the spicers and grocers on their part charged the apothecaries with similar malpractices.

As early as 1423 there is a record that the rector of the Grocers' Company and two apothecaries associated with him were empowered to search the shops of suspected apothecaries for adulterated drugs. When the drugs were found to be imperfect or adulterated, the apothecary was liable to be brought before the mayor and the drugs thrown into the street and trampled under foot. These dissensions and disputes went on until the reign of Henry VI, when letters patent were granted to the wardens of the Grocers' Company giving them the exclusive right of "garbling," which meant the cleansing, separating and examining of spices and drugs to detect and prevent adulteration. Certain drugs were required to be officially garbled before they could be exposed for sale, and this was generally done by the official garbler at the ports where they were landed. It is interesting to note that this law remained in force until the sixth year of Queen Anne's reign. The first record of this law being enforced occurs in the minutes of the Grocers' Company in 1456, where it is stated that a fine was imposed on one John Ashfeld "for makynge of untrewe powder of gynger, cynamon and sawnders." In 1561 also the Wardens record in their books "that bags and remantes of certain evil naghte pepper syrnamed gynger were to be burned." Many other entries of a similar nature show that the power of garbling was officially exercised, and as late as 1612 Mr. Lownes, apothecary to Prince Charles, complained to the Company that Michael Eason, a grocer-apothecary, "had supplied him with divers defective apothecaries' wares," and the offender was in consequence committed to the Poultry comptoir.

The last of the records referring to apothecaries in the fifteenth century is an entry in the Privy Seals dated February 16, 1462, where it is stated John Clerc, apothecary of the City of London, was appointed for life by King Edward IV to be the King's apothecary, receiving eightpence daily for his fee from the issues of the county of Devon and a robe of the King's vesture yearly at Christmas at the Great Wardrobe.

In 1562 apothecaries are mentioned as being "freemen of the Grocers' Company" as follows: "The Apothecaries, Freemen of the Company, are ordered not to use or exercise any drugs, simples or compounds, or any other kynde or sortes of poticarie wares, but such as shall be pure and perfyt good." These records show that even in the fifteenth and sixteenth centuries some attention was paid to the purity of the substances used in medicine.

No better picture of the apothecary of the sixteenth century has come down to us than that written by William Bulleyn, author of the "Government of Health," in 1558. Referring to the apothecary and his art and mystery, he writes:—

> He must fyrst serve God, forsee the end, be clenly, pity the poor.
> Must not be suborned for money to hurt mankynde.
> His place of dwelling and shop to be clenly, to please the sences withal.
> His garden must be at hand with plenty of herbs, seeds and roots.
> To sow and gather, preserve and kepe them in due tyme.
> To read Dioscorides, to know the nature of plants and herbes.
> To invent medicines, to choose by coloure, tast, odour, figure, etc.
> To have his morrters, stilles, pottes, filters, glasses, boxes, cleane and sweete.
> To have charcoles at hand to make decoctions, syrupes, etc.
> To keep his cleane ware closse and cast away the baggage.
> To have two places in his shop, one most cleane for the physik and and a baser place for the chirurgie stuff.
> That he neither increase or diminish the phisician's bill and kepe it for his own discharge.
> That he neither buy nor sell rotten drugges.
> That he peruse often his wares that they corrupt not.
> That he put not "quid pro quo" without advysement.
> That he may open wel a vein for to help pleuresy.
> That he meddle only with his vocation.
> That he delyte to reede Nicholaus Myrepsus, Valerius Cordus, Johannes Placaton, the Lubik, etc.
> That he do remember that his office is only to be ye physician's cooke.
> That he use true measure and weight.
> And lastly—To remember his end and the judgement of God, and thus I do commend him to God if he be not covetous or crafty, seeking his own lucre before other men's help, succour and comfort.

With this pious benediction we close these brief records of the apothecary down to the end of the sixteenth century.

DISCUSSION.

Dr. NORMAN MOORE said that the name of Bukerels still survived in Bucklersbury, a street on the west side of Walbrook, where the palace of the Bukerels once stood. Bukerels himself raised a regiment which helped to drive the Saracens out of Lisbon. The term "physician's bill" did not mean his charges for attendance but his prescriptions. The reason why the bedell's staves at St. Bartholomew's Hospital were coloured green and white was because these were the colours of the house of Tudor. In regard to the visitation of apothecaries' shops by the censors of the College of Physicians, he (Dr. Moore) remembered Dr. Farr, who actually took part in such a visitation within the City of London.

Mr. C. J. S. THOMPSON mentioned that in Ben Jonson's time Bucklersbury was still the street of the druggists.

Mr. J. H. FRANCIS NUNN referred to the "Fäldcher" of Prussia and Poland, and of some other parts of Germany. He said that an English lady, who lived for a long time in the towns of Poland and also in some country districts, often spoke of the "Fäldcher." From her he learnt that apparently the "Fäldcher" combined the good qualities of unqualified assistant, apothecary, and trained nurse. He apparently worked entirely under the direction of a medical man, except in remote country districts. The lady had called a consultant from Warsaw to see her for phlebitis, and part of the bandaging, &c., was occasionally done by the "Fäldcher." Apparently the "Fäldcher" generally gave satisfaction to the doctor and the patient, and his training was not so one-sided as that of the ordinary male nurse. He was also entrusted with slight surgical cases, but always under the control of the medical attendant.

Section of the History of Medicine.

January 20, 1915.

Dr. Norman Moore, President of the Section, in the Chair.

Exhibition of Books illustrating the History of Military Hygiene.

The President (Dr. Norman Moore), in the course of his introductory remarks, alluded to the practical value of the study of history, as illustrated in that of military hygiene. He was glad to think that the authorities had decided on composing a thorough and complete history of the present war, and had nominated competent men to deal with it. Their decision was interesting as showing the great advance which had taken place in our own country in the way of looking at history as a most important branch of study. Less than one hundred years ago Sir James MacGregor had collected, by assiduous toil, 345 large volumes of records of the history of the British Army. He sent them to the Public Record Office, hoping that they would be used as works of reference by all interested in the subject. About 1848 the authorities showed so little recognition of their value as to condemn them to be destroyed as worthless lumber. A memorandum of this order in the Annual Report of the Public Record Office is the only evidence that these 345 volumes ever existed. Thus there was a striking difference in the estimation of the value of history in 1848 and at the present day.

The earliest history of the kind was that of the American Civil War, an enormous contribution to military hygiene, medicine, and surgery. Nothing like it existed in English on a similar scale. The Americans also published several volumes of the same character on the Spanish War.

Various papers had been published concerning the Crimean and also the Boer War, but these were of the nature of enquiries and investigations rather than histories of these campaigns. In the time of the Crimean War the value of history as a scientific study was not recognised.

Fragments of history were contained in monographs such as George Cleghorn's "Diseases of Minorca," exhibited now, and also in Mr. D'Arcy Power's collection of letters from military surgeons. Cleghorn's was a very clear, though not exhaustive, account of diseases and conditions not previously observed. Possibly a more general history might be based on fragments such as these.

Of English writers on military hygiene, the first to be mentioned was William Clowes, Surgeon to St. Bartholomew's Hospital, and also to the Navy and the Army. In 1563 he served under the Earl of Warwick, at Rochelle, and in 1585 under the Earl of Leicester in the Low Countries. He was present at Zutphen, and was in the field when Philip Sidney died of his wounds. At the time of the Armada he gave up his post at St. Bartholomew's Hospital to become a naval surgeon. Soldier and sailor too, he gained experience in the wars in which he was engaged, and also took interest in the men who came back with Drake after his voyages. He well describes one who had been wounded by a poisoned arrow. Clowes was a general surgeon, but he also discussed at length questions of theory, such as, for instance, whether a bullet is rendered poisonous by the powder which discharges it, or merely inflicts a blow. He concluded that powder did not poison bullets. His book was worth reading on account of its excellent style, and the large number of facts which it contains—including a graphic description of the horrible torture and moral effects of red-hot irons employed to stay hæmorrhage. He also did good service in enlarging on the imperfect dietary supplied not only on ships but in London. We learn from him that scurvy was common at Christ's Hospital in those days, and that he attended boys suffering from that complaint every week. It was a curious fact that potatoes were not included in the dietary at Christ's Hospital until the last century, when they were introduced by Dr. George Budd. Clowes died in 1604.

John Woodall, author of the "Surgion's Mate," was appointed Surgeon to St. Bartholomew's Hospital in 1616, so that he was there in the time of Harvey. It had been incorrectly stated that Woodall's was the first book in which treatment of scurvy by lime-juice and fresh vegetables was recommended, but Woodall certainly advocated the use

of limes, lemons, tamarinds, and oranges as "helps to prevent scurvie and cure it when present." He was surgeon to the East India Company. He practised in London till 1643.

Richard Wiseman, Sergeant-Surgeon to Charles II, was a worthy professional descendant of Clowes and Woodall. He was at the battle of Worcester, and wrote a treatise on gunshot and other wounds, which had not the delightful freshness of Clowes, but contained an account of the wounds which he saw, whether they did well or otherwise, and what he did for them.

John Ranby, like Wiseman, confined himself to details of particular injuries, especially gunshot wounds, and his habit was to give fullest accounts when the wounded were great people—for instance, his report on the Duke of Cumberland's wounds formed quite a small book. He was King's Sergeant-Surgeon to George II and attended him at Dettingen.

Until the time of Charles I, who did a little to improve matters, the appointment of surgeons formed but a small part in military administration. Surgeons in the field were often but followers or vassals of great persons or of the King himself. They did what they could for the soldiers, but it was not much, as shown in Marlborough's campaigns. Surgeons were appointed to every ship, but not to every regiment. Physicians attended the army in the time of Marlborough and were consulted in cases of emergency.

Robert Pitt, who wrote a book on "The Craft and Frauds of Physick Exposed," stated that more than 1,000 men of Schomberg's army, before the battle of the Boyne, lost their lives owing to the badness of the *rhubarb* and other drugs supplied to the troops by contractors, who bought refuse drugs and sold them at considerable profit. Pitt's charges against these contractors were investigated by Lower and Millington. There seemed to be no doubt, however, that the deaths were due to enteric fever, for cases ceased to occur when the troops were transferred to districts where the water supply was better.

G. J. Guthrie served in the Peninsular War from the siege of Badajos to the end. His book on military surgery reads more like Wiseman's than a modern work. It includes many interesting cases, almost entirely surgical, which he observed, but only now and then general conclusions as to what might be done for the troops.

Richard Brocklesby, a man of great learning, was at school with Burke, and attended Dr. Johnson. He deserves mention as one of the

first to direct attention to the proper housing of troops in healthy barracks and places in time of peace.

The improvement in military nursing occupied a most important chapter in the history of military hygiene. Until the time of Florence Nightingale orderlies were the only nurses in the Army. It was Florence Nightingale who impressed upon the Government and the War Office that the sick and wounded in hospital would die in increasing numbers were not skilled nursing provided, and that an enormous number of lives might be saved by this means. It seems very evident now, but was not so then. Even at the time of the Boer War he (Dr. Moore) had been struck by the attitude of the earliest Director-General, who seemed to think it a waste of money to send out a great corps of trained nurses to the front. The experience of the Boer War abolished such notions, and it was now generally recognised that the provision of properly qualified nurses was a most important part of military hygiene.

The credit of awakening public attention to the subject belonged to Florence Nightingale, and it was extraordinary that she was able to overcome all the difficulties and enforce the adoption of the rational procedure which she advocated.

The Section is greatly indebted to Dr. Singer for the fine collection of editions of the chief works on Military Hygiene which he has laid before the Section, and special thanks are also due to Mr. D'Arcy Power for the loan of original manuscript collections.

Lucian and Medicine.

By J. D. Rolleston, M.D.

PART I.

There is no single representative of belletristic as distinct from scientific literature in the ancient world who throws so much light on contemporary medicine as does Lucian of Samosata. Although so much relating to medicine and its practitioners may be gleaned from his writings, no monograph on the subject, to my knowledge, is extant. I have, therefore, thought it worth while to collect the passages of medical interest in Lucian as I recently did in the case of the Greek Anthology.[1] Some events in his life are recorded in his writings, especially in "Somnium," "Nigrinus," "Bis Accusatus," "Piscator," "Alexander," "Hermotimus" and "Apologia pro mercede conductis," and internal evidence shows the period at which some of these works were written.

He was born at Samosata, near Antioch, about 120 A.D. After a very brief and unhappy experience as a sculptor's apprentice to his uncle he left Samosata to study rhetoric in Ionia. For some years he seems to have practised with success as an advocate at Antioch, but later his occupation consisted in composing speeches for others to deliver—of which "Hippias" or "The Bath," "Abdicatus" or "The Disinherited Son," and "De Dipsadibus," an essay on poisonous serpents, to mention only those of medical interest, are examples—and in giving lectures on rhetoric in various towns of Asia Minor, Greece, Italy, and Gaul.

In the course of his travels he visited the impostor and false prophet Alexander in Paphlagonia, and narrowly escaped assassination as the result of trying to expose his trickery. He spent some time in Rome, which he first visited to consult an oculist, for, like Horace, he suffered from some eye trouble of uncertain nature. His intimate knowledge of social life in Rome is well shown in "De mercede conductis," which contains several passages of medical interest. At the age of 40, having probably acquired by then a sufficient competence, he abandoned rhetoric, but before devoting himself entirely to moral and religious satire, for

[1] *Proceedings*, 1913-14, vii (Sect. Hist. Med.), pp. 3-13, and 30-56.

which he is best known, he passed through a transition period to which belong the "De Saltatione," a dialogue on pantomime, "Anacharsis," Toxaris, and probably the "Calumniæ non temere credendum," a treatise on slander, which all contain passages of medical interest. The best-known writings of the subsequent period which concern us are "The Dialogues of the Dead," "The Dialogues of the Gods," "The Dialogues of Courtesans," "Philopseudes," and "Alexander."

Towards the close of his life, "when he had already one foot in Charon's boat" ("Apol. pro merc. cond."), he moved from Athens, where he had been living hitherto, to occupy an important administrative post in Egypt. His death, which has by some been attributed to gout, on which he has written two burlesques, took place about 200 A.D., when he was aged 80, thus affording another example of longevity of which he had already recorded so many in the "Macrobii." The story related by Suidas, that his death was due to the bite of a mad dog as a punishment for his attacks on Christianity, is generally discredited.

The same difficulty attends the medical study of Lucian as in the case of the Greek Anthology. Many of the writings attributed to him are of doubtful authenticity and their date is therefore uncertain; among these are the burlesques "Tragodopodagra" and "Okypus," as well as "Macrobii," "Abdicatus," and "Alexander." The fact remains, however, that whether they are genuine or not, these writings contain, as I shall attempt to show, much that deserves the attention of the medical reader.

The same plan will be followed in the present paper as in my study of the Greek Anthology. The allusions to medical men, either as individuals or as a class, will first be considered, and notice will then be taken of the other medical allusions. In the preparation of this paper I am most indebted to the admirable introduction to the study of Lucian by M. Croiset, and to the French translation of Belin de Ballu, edited by Louis Humbert. I have also consulted the English translations of H. W. and F. G. Fowler and of A. M. Harmon. The edition from which the quotations are made is the Greek text published by Teubner in 1847.

Of the fourteen medical men mentioned by Lucian—viz., Agathocles (Cataplus, 6), Alexander (De morte Peregrini, 44), Antigonus (Philopseudes, 6, 21, 25, 26), Antiphilus (Toxaris, 27-34), Archibius (Gallus, 10), Callimorphus (Quom. hist. sit. conscr., 16), Ctesias (Vera hist., i, 3; ii, 31), Dionicus (Convivium, 1, 3, 20, 47), Hippocrates (Hermotimus, 1; Vera hist., ii, 7), Nicander (De

dipsad., 9), Paetus (Alexander, 60), Sopolis (Lexiphanes, 18-22), Soter (Okypus, lines 78 *et seq.*), and Toxaris (Scytha, *passim*). Hippocrates and Nicander are the best known; Callimorphus and Ctesias, though physicians, were better known as historians; Antigonus, Archibius, and Sopolis are spoken of by other writers, though their identity with Lucian's doctors cannot be determined; while the names of the other seven do not appear in any work which I have consulted.

Most of them seem to have been honourable if obscure practitioners, but one at least was a "shady" character. This was "Poetus, a physician by profession and a grey-haired old man, who did what was neither fitting for a physician or for one of his grey hairs," by seeking to be the successor of the impostor Alexander (Alexander, 60). Of the two references to Hippocrates one is a quotation of his well-known aphorism, "Art is long and time is fleeting," which Lucian says is even more true of philosophy than of medicine, while the other relates to the Telamonian Ajax, who is handed over to Hippocrates to be cured of his madness before he is admitted to the assembly of heroes in the Isles of the Blessed.

Some of Lucian's doctors seem to have been special friends of their patients, such as Dionicus, Archibius, and Antigonus, as they attended their banquets and took part in the conversation. Their freedom of speech towards their patients shows that their condition had considerably improved since the time when they were merely slaves in the Roman household. Thus the doctor Alexander, attending the philosopher Peregrinus who was suffering from fever and calling for water, refused to give it him, but told him that if he was so anxious to die death would come to him of its own accord, and that he had no need to sacrifice himself on a funeral pyre (De morte Peregrini, 44). Similarly outspoken language is used by Sopolis, who administers an emetic and purgative to Lexiphanes, and by Soter, who attends the gouty Okypus.

There are other doctors whose names are not given, such as the oculist whom Lucian consulted at Rome, the speaker in Abdicatus, and the medical attendant on the King of Assyria, whose diagnostic acumen in detecting the symptoms of love in a young man who feigned illness is amusingly told in the essay on the Syrian Goddess (De Dea Syria, 17-18).

I alluded in my last paper to the epigram variously attributed to Lucian, Lucilius, and Agathias, of the doctor who sent his son to a tutor but took him away again when he had only learnt the first three lines

of the "Iliad," saying that this lesson would be learnt at home, as he himself sent many souls to Hades and for that had no need of a tutor (Anth. Pal., xi, 401).

Apart from references to individual doctors, numerous passages, both laudatory and satirical, relating to the medical profession are to be found in our author. Some of the most interesting passages occur in the "Abdicatus" or "Disinherited Son." This is a rhetorical piece which appears in most editions of Lucian's works, though attributed by some to Libanius, a rhetorician of the fourth century, A.D., supposed to have been delivered by a medical man who had been disinherited by his father for refusing to undertake the treatment of his stepmother's disease, as to the nature of which we are left in doubt. The speaker relates how to enable himself to cure his father's illness, which seems to have been some kind of insanity, he had travelled abroad and after association with the most illustrious physicians in various countries had with much labour and zeal mastered the art of medicine. On his return he took his father's treatment in hand. "I did not," said he, "undertake to cure him at once, for that is not our custom, nor does our art advise us to do so, but first of all we are taught to see if the disease is curable or incurable and transcending the limits of our art. Then, if it is susceptible of cure, we undertake it and exert all our endeavours to save the patient; but if we see that the disease has got the upper hand we do not attempt to treat it, in conformity with the old rule of the forefathers of medicine, who say that one should not take patients in hand who have been conquered by the disease" (Abdicatus, 4).

Further quotations from the same piece testify to the high estimation in which the medical profession was held in the second century A.D. "The nobler and more useful for life it is, the freer it should be. Its liberty is one of its noblest privileges. It should be subjected to no violence or commands, but it is a holy thing, invented by the gods and practised by learned men. It is not under the yoke of the laws, it is inaccessible to fear, not subject to the punishment of tribunals, the threats of a father or the anger of a private person. . . . One must persuade the doctor, not command him; he should be induced, not intimidated, but should of his own free will undertake the conduct of a case. Our art is freed from paternal authority. It enjoys immunities of every kind. Wherever there are physicians cities bestow honours and the front rank upon them and grant them privileges and exceptions" (*ibid.*, 23).

Numerous other passages in Lucian illustrate the prestige enjoyed

by the medical profession. In three places (Quomodo hist. sit. conscr., 7; De Dipsad., 5; Philopseudes, 10) we find the term *ἰατρῶν παῖδες* or *Ἀσκληπίου παῖδες* to signify regular practitioners as distinct from the lay public or charlatans. Thus, in De Dipsadibus, 5, we read that the *ἰατρῶν παῖδες* explained the unquenchable thirst caused by the bite of the serpents on the theory that the naturally thick venom, on being diluted with water, becomes more active and rapidly diffused throughout the system. In the same piece Lucian says: "I have not wished to show myself a rival of Nicander or prove that I have carefully studied Libyan serpents. Such praise would be more suited for medical men who are obliged to know these things" (*ibid.*, 7).

Elsewhere we read that the sage Demonax reprobated vices but pardoned offenders, and urged that philosophers should imitate physicians who cure disease without being angry with the patient (Demonax, 7).

It is noteworthy that, though several references are made to oculists and eye salves, they are not of a satirical character as in Martial or the Greek Anthology. Mention has already been made of the oculist whom Lucian visited at Rome. In "Lexiphanes" one of the speakers says: "My sight is weakened, I am constantly blinking, and my eyes are watering and want a salve. I need a disciple of Asklepius, some oculist to make up a remedy to take away the redness from my eyes, clear their bleariness, and stop their running" (Lexiphanes, 4).

The philosopher Peregrinus, some days before his suicide, applied an eye salve which was so strong that it made his eyes water. The use of an eye salve at such a time seems to Lucian as absurd as if a man about to be crucified were to have a wound on his finger dressed (De morte Peregrini, 45).

The dangers to which doctors were exposed in the exercise of their profession are alluded to in more than one passage. Thus in Cataplus, 6, we read of the physician Agathocles accompanying to Hades his numerous patients who had died of fever. In the Convivium, 20, and Abdicatus, 16, the risks associated with attendance on the insane are described, and in Calumn. non tem. cred., 13, the medical man is given as an example of a victim of slander. "Slanders as a rule turn against their object the disadvantages he may possess and attribute to him crimes which appear probable. Thus a doctor is accused of being a poisoner, just as a rich man may be of seeking to be a tyrant."

It is not improbable that Lucian, like Cicero and Horace, had friends amongst the medical profession in Rome or elsewhere. Such

a supposition may explain the frequent allusions to medical subjects as well as his use of medical metaphors such as the following:—

Gnathon the parasite, after being struck by Timon, asks for money to staunch his wounds: "Gold," said he, "is a wonderful hæmostatic" (Timon, 46). In the Quomodo hist. sit. conscr., 7, we read that "History can no more tolerate a lie, even the smallest, than the trachea, as the medical faculty call it, can allow the entry of fluid." In the Apolog. pro merc. conduct., 2, one of the speakers, in submitting to cross-examination, says: "It is my place now to keep silence and submit to be cauterised, if necessary, and for you to apply the remedies and have ready the knife and the cautery."

The incisive language of a philosopher is described in similar terms: "The discourse of Nigrinus did not make a slight or superficial impression upon me, but the wound was deep and fatal and his well-directed words, so to speak, cut right into my soul" (Nigrinus, 35).

With the exception of the epigram of doubtful authenticity already mentioned, the satirical allusions to doctors in Lucian do not, as in the Greek Anthology, make any reference to thievish or murderous practitioners. Boastfulness, incompetence, and quackery are rather the vices which are stigmatised. In several passages the ostentatious and ignorant practitioner is contrasted with one of real skill and experience. Thus in Hippias, 1, we read that "A sensible man, when he is ill, does not send for the doctor who is best able to discourse about his art, but for one who has been longest in practice." In the piece entitled "Adversus Indoctum," 29, an uneducated man owning a large library is compared to "ignorant doctors who buy ivory medicine boxes, silver cupping glasses, and knives with gold handles, but when these instruments are needed do not know how to use them. It is then that a really experienced practitioner comes forward with a sharp though rusty knife and at once relieves the patient."

In the "Demosthenis encomium," 22, one of the interlocutors in the dialogue is compared to a doctor who had not understood what was the matter with the patient and was treating him for the wrong disease.

In the essay on "How to Write History" two physicians incur Lucian's satire, not for their medical but for their literary misdeeds. The first of these was Callimorphus, "whose fault was that he gave his book a more high sounding title than this mode of composition required —viz., "The Parthian History of Callimorphus, physician of the sixth legion of pike-bearers." He started with a very frigid preface, in which he declared that it was proper for a physician to write history, as

Asklepius was son of Apollo, and Apollo was leader of the Muses and master of all the sciences" (Quomodo hist. sit. conscr., 16). Elsewhere in the same treatise (*ibid.*, 39) Lucian attacks Ctesias, the author of a Persian history, and attributes his unveracity to his being physician to Artaxerxes and to a desire to obtain a substantial reward for his fulsome panegyric.

The prevalence of charlatanism is illustrated in several passages. In the two Syrians from Damascus (Tragodopodagra, 265 *et seq.*) we have examples of itinerant quacks, though dignified by the name of physicians, who under the pressure of poverty and hunger have brought with them a secret remedy for gout bequeathed them by their father on his death-bed, with the strict injunction never to reveal its composition.

In the "Apologia pro mercede conductis," 7, a man whose claims are unwarranted is compared to a quack, who while boasting of a remedy for a cough is suddenly interrupted by a violent paroxysm himself.

The palm in quackery, however, is to be given to the impostor and false prophet Alexander, to whose adventurous career Lucian has devoted one of the most entertaining of his descriptive pieces. Alexander was born in Paphlagonia, a region notorious for its simpletons, and was brought up by a man who was professedly a physician but was probably a poisoner, a friend of Apollonius of Tyana. Possessed of all the physical advantages a charlatan could desire, a lofty stature, a handsome face and commanding appearance, a piercing eye and a pleasant and sonorous voice, Alexander found his fellow-countrymen an easy prey. With a dancing master as his accomplice, he buried two brazen tablets in a disused temple of Apollo with the inscription that Asklepius, accompanied by his father, Apollo, was coming and would take up his abode at Abonoteichos. The tablets were soon discovered and by clever juggling with some tame snakes he convinced the populace that a new Asklepius had appeared. He gave himself the name of Glycon, simulated divine inspiration by wild gestures and filling his mouth with foam obtained by munching soapwort, and for the modest sum of 1 drachma and 2 obols (about tenpence) made oracular responses to all who consulted him about their health or other matters of importance. His fame rapidly spread to the adjacent provinces of Bithynia and Galacia, and finally over the whole Roman empire, and his annual income reached the respectable figure of 70,000 drachmas (about £2,880), by which he was able to defray the expenses

of a large staff of assistants, spies, clerks and interpreters. Messengers were sent to every country to announce that he cured all diseases and even raised the dead. His early training had given him some knowledge of medicine, but the remedy on which he set most store was a nostrum composed of goat's fat which he seems to have used as a panacea. As might be expected, his prognosis was not always correct, but he safeguarded himself by producing oracles after the events. Thus, when patients died whose recovery he had pronounced certain, he produced another oracle couched as follows: "Seek not a cure for your dire disease. Your fate is certain and it is impossible to escape it." He did not even hesitate to fortell a visitation of a pestilence and promised to avert it. Unfortunately for him the houses on which he had had inscribed the promise, "Phœbus of the unshorn locks will keep the pestilence far from you," were precisely those which were stricken with the plague. Attempts to expose his trickery were met by the retort that his opponents were Epicureans or Christians, both highly unpopular sects. Lucian himself only escaped assassination by the softheartedness of the sailor whom Alexander had ordered to drown him. The impostor's end was a fitting one. Though he had prophesied that he would live one hundred and fifty years and be killed by lightning, he died at the age of 70, "eaten of worms," like Herod, and of "putrifaction of the foot extending to the groin" (probably senile gangrene). It was not till his last illness that it was discovered that he was bald, for the severe pain in his head compelled him to remove his false hair.

Confirmation of the edifying story of one whom Froude has aptly called a "Cagliostro of the second century" is provided by the coins minted by Alexander, of which Lucian speaks, being still extant. They bear on one side the image of Glycon and on the other Alexander himself, wearing the crown of Asklepius and holding the sword of Perseus from whom he claimed to be descended on his mother's side.

The success of Alexander's impostures is comprehensible when we realise that superstition, especially in therapeutics, was particularly rife at that time. That credulity was not confined to ignorant folk such as the Paphlagonians, but was prevalent also among the cultured classes, is admirably illustrated in the "Philopseudes." In this dialogue one Tychiades, whom we may regard as the mouthpiece of Lucian, relates how he paid a visit to his friend Eucrates, whom he found suffering from what was apparently an attack of gout, and surrounded by his friends, including the doctor Antigonus. All were discussing the disease

and suggesting a remedy. "If," said Cleodemus, "you lift from the ground with the left hand the tooth of a weasel killed in the way I told you and tie it up in a freshly flayed lion's skin and bind it round the leg, the pain stops at once." "Not in a lion's skin," said Dinomachus, "but in the skin of a hind that has not been covered. This is much more comprehensible, for the hind is swift and its chief power rests in its feet. The lion, it is true, is strong, its right foreleg and the hairs of its mane have great effect if one can use them with the appropriate incantation, but they have no power to cure the feet." Tychiades then exclaims: "Do you believe that pains can be stopped by charms and amulets when the disease is internal?" They laughed at this, and showed that they condemned his ignorance of what was so obvious, such as no sensible person would contradict. The doctor Antigonus, however, seemed very pleased by the question, for he had long since given up treating Eucrates with the help of his art by ordering him to abstain from wine, to eat vegetables, and to refrain from over-exertion. Cleodemus then remarked with a smile: "What, Tychiades, do you doubt that such things are of help in disease?" "I do indeed," replied Tychiades, "I should be a great simpleton (literally, "my nostrils would be bunged up with mucus") if I believed that remedies applied externally and having no connexion with the internal causes of disease could be effective by virtue of certain words or enchantments. Such things could never happen, even if sixteen hinds were tied up in the skin of the Nemean lion. Personally, I have often seen a lion limping with pain, although covered with its whole skin." "You are very silly," said Dinomachus, "in having neglected to learn these kinds of remedies and their mode of application in disease. You do not appear to admit what is most obvious, such as the driving away of periodic fevers, the charming of snakes, the cure of inguinal tumours and other things which old women do." Dinomachus then accuses Tychiades of atheism, but Tychiades refutes the charge with these words: "I reverence the gods and see their cures and the good they do to sufferers by drugs and the art of medicine. Asklepius and his sons cured their patients by mixing soothing balms and not by applying lions or hinds."

Further miraculous cures are told by the others present, and the doctor Antigonus relates the following story: "I have," said he, "a bronze statue of Hippocrates, about a cubit high, which whenever the lamp is extinguished runs noisily up and down the house, upsetting the medicine boxes, mixing the drugs together, and opening the doors,

especially if we delay the sacrifice which we offer him every year." Subsequently Antigonus relates how he attended a man who came to life again twenty days after he had been buried—probably a sneer, as de Ballu suggests, at the resurrection of the Pamphylian ER whose story is told in the tenth book of Plato's Republic (614B *et seq.*).

The Pythagorean philosopher Arignotus then appears, and after joining the chorus of indignant protest against the incredulity of Tychiades relates a story even more fantastic than those which had gone before. Finally Tychiades makes an excuse for leaving the company: "After what I had heard," said he, "I felt my stomach blown out like one who had drunk sweet wine, and I needed an emetic. I would gladly have paid a large sum for a medicine to make me forget all I had heard."

(To be continued.)

Section of the History of Medicine.

February 17, 1915.

Dr. NORMAN MOORE, President of the Section, in the Chair.

Jean Astruc.

By Emeritus Professor Sir ALEXANDER SIMPSON, M.D., D.Sc., LL.D.

"THIS work was composed some time since, but I hesitated to publish it, fearing that the self-styled free-thinkers who seek support on all sides might make an ill-use of it for lessening the authority of the Pentateuch. A learned man, very zealous for religion, to whom I showed it, dispelled my scruples. He has assured me that what I supposed about the documents of which Moses had made use in composing Genesis, had already been advanced in essence by several authors (such as the Abbés Fleury and le François) in works well approved, that the special application I was making of the suggestion in distributing Genesis in several columns, representative of these documents, made no change whatever in the text of the Book of Genesis, or made no more change than what had been done in dividing it into chapters and verses; and that so far from being in any way prejudicial to religion, it could not fail, on the contrary, to be very helpful in serving to remove or clear up several difficulties which present themselves to the readers of the book, and under the weight of which commentators have been hitherto almost overwhelmed. On his advice, then, I have decided to publish this work and submit it to the judgment of the enlightened, whose observations I will be pleased to hear. I protest beforehand very sincerely that if those who have a right to criticise, and whose criticism I ought to respect, find my conjectures either false or dangerous, I am ready to abandon them; or better, I abandon them now. Preference for my ideas will never prevail with me over the love of truth and of religion."

So runs the preface to a small volume (525 pp.) written in French

and published in 1753, of which the Advocates, the University, the New College, and the Royal College of Physicians of Edinburgh have each a copy in their respective libraries. The title-page tells that it contains *Conjectures sur les Mémoires originaux dont il paraît que Moyse s'est servi pour composer le livre de la Genèse.* Avec des Remarques qui appuient ou qui éclaircissent ces Conjectures." It bears the motto:—

> "Avia Pieridum peragro loca, nullius ante
> Trita solo."

The author's name is not given; but it was well known to have been written by a physician, Jean Astruc, as the respective librarians have noted.

The Book.

From the book one gathers that the author was an accomplished scholar, well versed in various languages and accustomed to read the Bible in the original. It does not occur to him to doubt that the Pentateuch came from the hand of Moses and that it is all to be accepted as part of a divine revelation. But he uses his imagination. He wonders how Moses knew all that he has set down here. Moses' own memory could only go back to the things he treats of when he gets into Exodus. How did he know about the creation, the deluge, and the rest of the Genesis record? Either he must have been taught by special revelation, or he learned them from those who could tell the stories. Astruc does not know of any who hold by the first of these alternatives. Moses, though in other parts of the Pentateuch he claims, like later prophets, direct divine authority for his messages, speaks throughout Genesis simply as a historian. It must be supposed, therefore, that he derives the events he records from ancestors who during the 2,369 years of the Usserian reckoning had successively witnessed them and passed on the narratives to their descendants. "All the same," says Astruc, "it must also be admitted that Moses was enlightened in a special way, and by inspiration in his choice of the facts and attendant circumstances he had received from his forefathers, and that we owe to the history he has bequeathed to us the foundation of the Divine Faith."

The suggestion is discussed that the stories have come down through "Shem who had seen Lamech, who had seen Adam, and who had himself been seen at least by Abraham, who had seen Jacob, who had seen those who might have told Moses." But he sets it aside in favour

of the idea, already entertained by Le Clerc, Simon, and others, that the Hebrew race were in possession of a series of written memoranda dealing with their origin and history. These Moses collected and may be supposed to have kept them in portions according to the incidents related, each in its entirety so that nothing should be lost. Hence repetitions, with variations of the same story; hence some anachronisms as to the order of events; hence the predominant use in one passage of Elohim, the common name of God, and in others of Jehovah, "God's great name, the name which expresses his essence." The Jews shrank from pronouncing this name, and read Adonai instead, and put the vowel points of Adonai under the consonant of the sacred name. If Genesis be decomposed according to the prevalence of these names into distinct sections, placing those with Elohim in a column A, and those with Jehovah in a column B, the differences in the documents become clearer. Then it might be necessary to have a column C for a few facts recorded thrice, and a fourth column D for passages outside the proper Hebrew history and where no name of God is employed. There may be further subdivisions and arrangements of columns going as far as L or M. But Astruc thought the construction of the book would be sufficiently indicated by the use of the A, B, D columns, which he accordingly proceeded to arrange with the gaps in successive chapters in one column filled up by the verses to be found in the corresponding chapter of the neighbouring column. It would not have been intelligible to many if printed in Hebrew. The Septuagint and Vulgate versions, and translations from these, have not always rendered the names definitely enough. The Geneva version, a direct translation from the Hebrew, gives consistently the general name for God as its rendering of Elohim, while for the essential name it adopts the sufficiently expressive rendering "the Eternal." So he reproduces the Geneva version of Genesis in this columnar form; and afterwards goes on to show how such a view of the book clears away the various difficulties which have been a burden to commentators and weapons of attack for opponents of the faith. He wishes his readers not to make up their minds on a first reading. "When we have to undo a prejudice in which we have been brought up, we must accustom ourselves gradually to the opposite opinion and give time for the reasons for embracing it to act upon us, for pre-occupation (*prevention*) never yields but with difficulty and counterbalances for long the strongest proofs." It is all done on the intellectual plane, reverently towards its subject, respectfully towards the reader.

The Man.

What manner of man was Jean Astruc? I first met him in his latest work, *L'Art d'Accoucher réduit à ses Principes*, published in 1766, the year in which he died. Here he styles himself "*Professeur Royal du Médecine, et Médecin Consultant du Roi.*" He was a man, then, in the highest rank of his own profession. Of a well-connected family he was born in 1684, at Sauve, in Languedoc, in a Huguenot manse, and baptised in a Protestant Church. But he never knew himself as other than a professed Roman Catholic. Whilst he was but a child his father abjured his Protestant faith, took to the profession of law, and devoted himself to the education in literature and philosophy of his two sons, who never had any other teacher. The younger of the two died at a comparatively early age after a brilliant career as an Orator and Professor of French Law in the Faculty of Toulouse. Jean studied medicine at Montpellier, where he "sacrificed to the pleasure of study and observation all the amusements of his age and even the pardonable delights of Society"; published his first dissertation while only a Bachelor of Medicine, in 1702, obtained the same year the licence in medicine, and in 1703 the M.D. degree. In 1710 he won by competition the Chair of Anatomy and Medicine at Toulouse. In 1717 he filled a chair in Montpellier. The King of Poland called him to be his physician in 1729; but the following year he went to Paris, where libraries were accessible to him for the prosecution of his labours. Almost immediately he was honoured with the title of Consulting Physician to the King, and in 1731 was appointed to a Chair of Medicine in the Royal College. His lectures were marked by such graces of style that many people who had no interest in medicine attended them as models of beautiful Latinity. He was a ceaseless worker. "He might be seen in the coldest winter night, at an advanced age, studying at three in the morning without a fire by the light of a lamp." The Faculty of Medicine honoured him in 1743 by coöpting him unanimously as one of its members. "This unanimous vote of enlightened men, more precious to a man of science than all the favours which fortune sometimes flings indifferently to merit or to ignorance," Astruc much appreciated. He took a constant interest in the work of the Faculty and "visited the poor patients who came for advice on Saturdays as diligently as if he had nothing else to do." So he kept on working in old age, and though his infirmities

were aggravated by his great grief at the death of his only daughter the year before his own decease, he took an active share in the researches of a commission appointed by the Faculty to report on inoculation, and died in 1766, at the age of 82.

As he appears in his Works.

This and much more is recorded of him in a biographical sketch prefixed to a posthumous volume of *Mémoires pour servir à l'Histoire de la Faculté de Médecine de Montpellier*, edited by his friend Dr. M. Lorry, who explains that the infirmities of advancing years led to his publishing some of the works such as the *Conjectures* for which he had long been collecting material. In the preface to his *Histoire Naturelle de Languedoc*, Astruc says he hesitated long about the publication, "because it seemed to me that it was not becoming in a person engaged in a very serious and very exacting profession to take himself up with researches in pure physics or literature. But at last love of country has carried me past this scruple, and I have let myself be persuaded that my present occupations do not prohibit the publication of a work composed long ago." This work has three divisions—geographical, physical, and literary. It is full of conjectures, as, e.g., in regard to the positions of places in old itineraries, and his knowledge of Hebrew enables him to correct mistaken conclusions that have been drawn from the itinerary of Benjamin of Tudela.

The preface to his Midwifery Manual[1] begins by explaining that he had never attended a case of labour! But having undertaken at the urgent desire of the Faculty of Medicine to deliver in 1745 a course of lectures to midwives, and being given six months in which to prepare, he set himself "to read or re-read all the treatises on the Art which had appeared in French or Latin within the previous thirty years." The course was a success. It was continued two years more with "new lectures, new extracts, new reflections." He had laid aside these Lectures on Midwifery until it was represented to him that, although the midwives in Paris and some other large towns received instruction, throughout the provinces generally they were untaught and might profit by the reading of his lectures. Still, he did not make up his mind till it occurred to him that it was the necessary complement to

[1] "Je multiplierai vos douleurs et vos grossesses; vous mettrez au monde vos enfants dans la douleur. Genèse, iii, 16," is printed on the back of the title-page.

a more important treatise[1] he published on the Diseases of Women. Needless to say, neither treatise is of value nowadays, either from a scientific or a practical point of view. But Astruc had a keen delight in laborious historical research, and he devoted sixty pages to a history of the Obstetric Art. About thirty years ago, at the instance of Principal Sir Alexander Grant, I drew up a history of the Edinburgh Chair of Midwifery, which proved to have been the first instituted in any University. Inquiring into the causes and date of the transference of obstetric practice from women to men, I had to quote Astruc as the recognised authority for the explanation that it was all a matter of fashion, resulting from the secret and successful management by a surgeon, at the instance of Louis XIV, of the delivery of Madame de la Vallière, after which the princesses all wished to be attended by surgeons, for whom the designation of "Accoucheurs" was coined and adopted into other languages. Having referred to his eminent position as a medical authority I added that "by pointing out the distinction between the Elohistic and Jehovistic passages in the Old Testament, he opened up a path for theologians along which the higher critics of our day have travelled to a length that would have greatly astonished him."

My justification for that remark rests on the acquaintance I had made many years earlier with an appendix to this same manual, called on the title-page *Une lettre sur la conduite qu'Adam et Eve dûrent tenir à la naissance de leurs premiers enfants.* I was working at a dissertation for the Royal Medical Society on the umbilical cord and its torsions, which I afterwards presented as my thesis for graduation with the motto—

> "Deus aethere missus ab alto
> tortos incidere funes
> Ecce iterum stimulat,"

and among other readings came upon Astruc's letter. I had no occasion to make use of it, though it may have suggested the motto. But the memory of its quaint conceits and the näive suggestion that God himself came down to give Adam and Eve the first lesson in midwifery remained with me. It has this interest that it is the last output of all Astruc's activity.

The letter, eighteen pages in length, was occasioned by the quandary

[1] In the preface he has a sentence "against blindly following our own imaginations and substituting conjectures, hypotheses and mere suppositions for truths."

into which his friend, Mr. F. D. B., was thrown by a "philosopher[1] of the time," who propounded to him a puzzle as to how Adam and Eve comported themselves on the birth of their first children. Did they tie and cut the umbilical cord as we now do? How did they know what to do? Who taught them? They had been created without an an umbilicus and had never seen a child born. Did they not tie and cut,[2] then the children must all have died, and there was an end of the race? "You tell me, Sir," writes Astruc, "that this objector has embarrassed you, and you beg me to show you how to reply. But you seem to me to be greatly shocked with the air of complacency and the tone of mockery with which it has been proposed to you. Don't you know that, that is the way with these gentry? Full of the sublimity of their lights, they think that the least difficulty they bring forward ought to upset truths the most worthy of respect. But they do not long enjoy this vain triumph. Answer them, and they are confounded. It is the case of him of whom Horace says :—

> 'Qui fragili quaerens illidere dentem,
> Offendit solido.'

"This is exactly the case of your philosopher. Nothing more frivolous than his objection. I send you three or four answers, that he may have his choice of them. They are all plausible. I might say they are all solid."

(1) "Adam must have been greatly surprised to see the after-birth attached to Cain by the cord, but he let it alone, till in five or six days it fell off. He profited by the observation, and when Abel was born, he cut the cord. Seeing some blood escape he tied it, and so from the birth of the second child the deligation and division of the umbilical cord began to be practised by which the human race was saved.

(2) "Adam while still in Paradise had given names to all the creatures and must have observed how some of them gnawed the umbilical cord of their young at birth without loss of blood. When his wife, expelled with him from the earthly Paradise, began to give birth to children he may have cut through the cord with his teeth, as

[1] A French variety of the species of which Addison sketches a specimen (*Spectator*, September 5, 1712), who sets up for a free-thinker and is an atheist as much as his parts will give him leave. Mr. B. invites Astruc to answer his philosopher as Will Dry was called on to shut up Tom Puzzle.

[2] He has noted elsewhere that by the Greeks the practitioners of midwifery were called ὠμφαλοτόμοι.

savages used to do when the French first landed in Brazil. If any bleeding followed he would naturally tie.

(3) "I go even further, and I suppose that Adam, disliking the sight of placenta and cord hanging from Cain pulls them off. What then? 'Certain death to Cain,' says your philosopher. Not at all."

Through a series of pages of reasoning and citation of authorities, Astruc shows that such an accident is not necessarily fatal, and concludes that Adam may so have treated Cain without harm and without the loss of the human race.

F. D. B. might give the philosopher his choice of any of these solutions of his difficulty. "They are all plausible and conclusive. For myself, Sir, I do not adopt any of them, but I believe you can answer your philosopher in a manner more general and decisive."

(4) "I believe that God, who taught the birds to build their nests, and the quadrupeds how to deal with their young, taught Adam what to do to save his children at birth." "It is certain that he deigned to give instruction to men at the beginning of the world in matters of much less importance." He had taught Adam whilst still in the earthly Paradise to speak, so as to be able to name the animals: he taught Cain how to till the ground and Tubal-cain how to work in metals. "Why should we not say," he repeats, "that God had taught Adam what was needful to be done for the preservation of his newborn children, supposing he had left anything to his care."

(5) Astruc would have his correspondent round upon his assailant. "These gentry think themselves quite strong in their attacks: they are quite weak when they are put on the defensive." Either, the race began by the creative will of God; or, it exists of necessity and from all eternity. If the philosopher admits the first alternative, he only differs from the Church in giving an earlier date to the origin of the race and his difficulty remains. To adopt the other alternative is to admit the palpable absurdity of a *necessary series of contingent individuals*, which is a manifest contradiction. Either, the men of this series have learnt by repeated observations the necessity of tying the cord, and, in that case, before they had acquired the knowledge the race would have had time to perish, or this knowledge was in them necessary and innate. Thus you have a new absurdity; which need not surprise us, for absurdities evoke one another. Admit for the first man of the created series the same knowledge as innate but not necessary, for it was given by God. So the assailant is foiled with his own weapons, despoiled of their impiety.

"Show this letter, Sir, to your Philosopher. If he is willing to read it with care, I hope it will shake the confidence he has in his opinions. But I desire that my reflections may have a yet happier success and that they may guide him to fair reason and persuade him to have more respect for the truths of revelation." This, I note again, is the last expression we have of Astruc's active mind.

If we look back into some of his other writings we get just such an impression of the man as arises from an unprejudiced reading of the preface to his *Conjectures*. His greatest and best known work is that on Venereal Diseases written at first in Latin. It has been a mine of information for syphilologists interested in historical research. As regards the origin of this disease he strongly advocates the opinion that it was of recent development. Quoting in the preface from Lucretius—

> "Medio de fonte Leporum
> Surgat amari aliquid,"

he says, "In the midst of sensual pleasure there arises something bitter, nay very bitter, which plague, neither seen nor heard of in ancient times seems lately to have escaped out of Pandora's box, as a bitter dreg reserved for this wicked age, either by way of bridle to restrain our ungoverned lust or of a whip to punish it." In one of his notes he hopes he will never come under the censure of Scribonius Largus "who, although he was a heathen, expressed himself so much in the manner of a Christian that he may rise up in judgment with not a few of the Christians and condemn them. Such (says he) 'as discover the usefulness of certain remedies and keep it a secret are very much to be blamed, being guilty of the crime of envy, which ought to be hated by all mankind, but especially by physicians who if they do not abound with that compassion and humanity which ought always to attend the profession, deserve to be detested both by gods and men.'"

In book 1, chap. iv, he declares that "It is much against my inclination to bring in the Sacred Scriptures into a subject of this kind, but I am forced to it by those who contend for the antiquity of the venereal disease, who omit nothing that can be thought of in order to support their opinion, while they hope to cull anything out of them in favour of their subject. But they only expose their ignorance by affecting too much knowledge and show more learning than judgment in quoting those very passages which, if examined with sufficient diligence and accuracy are plainly rather against them than otherwise." Then follow discussions through a series of paragraphs in his orderly

fashion on (1) leprosy, (2) some references in the Mosaic Laws, (3) Job's boils which they interpret to Job's discredit, and which Astruc takes to be of the nature of elephantiasis, and (4) "They are at a great deal of pains, and labour very assiduously to asperse David with that scandal of which we have cleared Job." Astruc agrees with the majority of commentators that the Psalmist's complaint "ought to be understood, not of bodily sickness, but of the mind diseased by sin: . . . . . ought not to be taken in a literal sense, as relating to complaints of an infirm and sickly body, but to the tortures of a grieved and penitent mind." In his history of Montpellier he has occasion to speak of Rabelais and censures his "impious allusions to the words of the sacred books and profanation of what is most to be respected in religion."

Professional Estimates.

We may gather the impression made by Astruc on his medical brethren from the eight columns (the conjoint work of Chaussier and Adelon) that are devoted to him in the *Biographie Universelle Ancienne et Moderne*, 1811. Referring to his having been trained at Montpellier, where the misleading mechanical system of Boerhaave had displaced the chemical theories of Silvius, the writers say, "Astruc, whom Nature had endowed with a mind upright (*esprit droit*) but not active enough of himself to preconceive any high truth, followed the false direction, and in all the course of his long and laborious career, if he showed himself profound and able in the science of books, he was backward in the rigorous way of observation and research, in a word his speculative and practical medicine in place of being Hippocratic and vital was entirely mechanical and mathematical. However, few men won so great a reputation among his contemporaries. Astruc, as regards theory, owed his success to his natural eloquence and to a spirit of method, which setting him to divide the subject he ought to develop made him move from one division of his theme to the other by rigorous definitions. This made him a fascinating professor, whilst his zeal for study and his prodigious memory made him an erudite man. As regards practice, his reputation was due to a spirit of reserve and circumspection which usually led the medical practitioner to a wise expectancy by which he replaces the precious but rare quality of medical tact. Astruc had not in theory any of the large views which go to the deep foundations of a philosophy of medicine, nor, in practice, the rapid and sure perception which judges at once and without the risk of the least error the move-

ments of Nature in diseases. This, in fact, is not to be found in any of his numerous writings."

The same biographers speak of him again as "endowed with a prodigious memory, an upright mind, but cold and not very original, capable rather of receiving an impulse than given to publish it, but that he established 'order in his vast works of erudition.'" He engaged in various medical controversies, among others on the nature of digestion with Professor Pitcairn, of Edinburgh, as to whether it was more a physical (Pitcairn) or a chemical (Astruc) process. Each went wrong in his negation. Referring to this contention, in which I have to confess that my countryman wrote of Astruc in very unparliamentary language, they say "His writings may at least be cited as a model of courtesy in controversies." As to the treatises that specially interest theologians, they say, "We have said that metaphysics was one of his favourite occupations. In 1753 it inspired him to write *Conjectures*, and soon thereafter, to dispel the doubts his work had raised regarding his orthodoxy, he published two dissertations on *Immortality* and on the *Immateriality* and *Liberty of the Soul*. The *Nouvelle Biographie Générale*, 1854, says of Astruc, "Love of work and desire to increase the domain of science prevailed above all other sentiment in him. He won without seeking them honour and fortune, and sacrificed everything to the taste that dominated him."

DISCUSSION.

The PRESIDENT (Dr. Norman Moore): Astruc is chiefly known to medical men as the historian of the Faculty of Medicine at Montpellier—but there were many lines of thought with which his name was connected. The suspicions entertained by worthy theologians as to Astruc's moral character may be dismissed. He was a man of great industry and of an inquiring mind; a student of local history and of the natural history of races, and of languages, especially that of Languedoc. Beloved by students, respected by all in Toulouse, he was elected by the ruling classes of the country, whose liberties he defended, to the important position of captal. These are not the materials of which profligate villains are made, and in this country at all events people are assumed to be innocent until found guilty. A point of interest in Astruc's medical career was that he first took the degree of B.M., then became a Licentiate in Medicine, and finally an M.D. Such a sequence is quite unfamiliar at the present day, but it was customary at Cambridge until fairly recent times. The late Dr. Robert Martin followed exactly the same course.

Dr. J. SKINNER (Westminster College, Cambridge), after thanking the Section for the opportunity afforded to visitors of hearing a most instructive lecture, said: I approach the subject from the point of view of a Hebraist and an Old Testament student. I am interested in Astruc as the discoverer of the clue to the analysis of the Pentateuch in the alteration of the divine names, Jehovah and Elohim, in the book of Genesis. Scholars generally are aware that Astruc stood in the front rank of the medical profession of his day, but his work in that department and the range of his mental activity are perhaps little known among them, and they naturally welcome any light that can be thrown on his character and reputation. The fact that a discovery of such epoch-making magnitude was due to an amateur in biblical criticism is a convincing proof of great acumen and originality on the part of its author: at the same time it is one of several instances in the history of science—for example, Priestley's discovery of oxygen in 1776—where an untrained mind, free from the prepossessions of the specialist, was successful in forming a theory which satisfied experts, although it did not occur to any of themselves. There are two points on which I should desire clearer knowledge. One is the attack on Astruc's morals, made many years ago by Professor Osgood in the *Presbyterian and Reformed Review*. I am inclined to accept the Chairman's view, that Astruc's record of intellectual activity is a sufficient defence against the charge of profligate living. The second point is as to Astruc's competence as a Hebrew scholar. In a recent controversy on the critical value of the divine names as an instrument of analysis, it was alleged that Astruc was totally ignorant of Hebrew, and based his investigation solely on his French Bible. Even if that were true it would detract nothing from the importance of his discovery; for the Geneva version which he used correctly represents the divine names of the Massoretic text, and was as safe a guide for Astruc's immediate purpose as the Hebrew text itself. Still it is reassuring to learn from the lecture that Astruc had a sufficient knowledge of Hebrew to be able to study the documents in the original. It has been a great pleasure to listen to Sir A. Simpson's lucid and informing paper.

Dr. I. ABRAHAMS (Christ's College, Cambridge): Astruc's "Genesis" is well known to students of the Hebrew Scriptures, but Sir A. Simpson in his charming discourse has revealed Astruc's personality. The charge that Astruc was morally a "profligate" was a not unusual controversial device, just as that earlier Bible critic, Spinoza, had been assailed for the opposite fault, his supposed cold, austere, passionless detachment from the ordinary interests of men. It would be well, however, that the charge against Astruc should be fully probed. As to his work, it has stood the test of time wonderfully, and the "documentary" theory of Genesis still holds the field. Very important, moreover, is the fact that even if Astruc's criterion (the use of the divine names) had been weakened (which is not the case, as Dr. Skinner has shown), his work would nevertheless be assured of lasting fame, in that it has been the cause of a vast amount of fruitful research on the structure of the Pentateuch.

Dr. A. E. GARVIE (New College, London): I thank the Section for the opportunity of hearing so interesting a paper as that Sir Alexander Simpson has just submitted, and I would refer to four points suggested by it: (1) The views on midwifery of Astruc might well make one doubtful of his judgment on other matters, did not these views depend on the knowledge of his age, while his *Conjectures* on the composition of Genesis had their basis in observed facts. (2) The attack on his character to discredit his initial opinions could only undermine the position of those who stooped to use such weapons, as argument should be met by argument and not by insinuations against the character of an opponent. (3) The opponents of these critical views are only too ready to snatch any weapon, however dangerous it may be to those who use it, the latest weapon being a view which brings the value of the Massoretic text of the Old Testament into doubt in a very important respect—the use of the divine names. (4) Astruc disproved the wisdom of the saying that "a shoemaker should stick to his last," as he had made a more valuable contribution to biblical scholarship than to his own science of medicine. While I should not even venture to suggest that medicine could learn anything from theology, I gladly acknowledge the debt theology owes to medicine in Astruc.

Sir WILLIAM OSLER: The literary output of a long life shows Astruc to have been a hard-working, painstaking student, and we are all very much indebted to Sir Alexander Simpson for reminding the profession of the important position he occupies in the history of Biblical criticism. Some months ago I was shocked to read in an address by the Rev. A. C. Dixon on "The Bible and the Origin of Life," reported in the *Times* of June 14 last, the statement that the father of modern Biblical criticism, though a man of considerable learning, was of profligate life. In the medical biographies there is no hint of anything to justify such a remark, but Sir Alexander Simpson called my attention to an extraordinarily bitter criticism of Astruc by Professor Howard Osgood in the *Presbyterian and Reformed Review*, January, 1892, vol. iii. The main contention is that he was the intimate friend and paramour of Madame de Tencin, perhaps the most famous of the infamous Court circle of that day and the mother of the great d'Alembert, whom as an infant she had deserted. At her death all her property went to Astruc. I must say Professor Osgood tells a hard story of Astruc, but it is chiefly of his associations, and the sources are drawn from bitter enemies of the Jesuits, with whom Astruc was associated. And we must remember what Montaigne says, that a man may sail over troubled waters without fishing in them. About the *Conjectures*, Professor Osgood quotes from Voltaire a statement that Astruc and his son-in-law, Silhouette, who was a candidate for some Government position, bought up all the copies in the kingdom, "and that is the reason why they have become so rare at present." Altogether, I feel that we should accept the charitable opinion of our President on the distinguished author of the *Conjectures*.

Lucian and Medicine.

By J. D. Rolleston, M.D.

PART II.

In the first part of my paper[1] I dealt with the allusions made by Lucian to individual doctors and to the medical profession as a whole, and gave several quotations illustrating the prevalence of charlatanism and superstitions relating to medicine. In the present communication I shall draw attention to other medical allusions in Lucian's writings —viz., to medicine and religion, especially as regards Asklepius, to prevalent diseases, especially alcoholism, gout, malaria, and febrile disorders, and to other subjects more or less closely connected with medicine, such as baths, cosmetics, physical education, and longevity.

Medicine and Religion.

Numerous allusions are made to Asklepius, the tutelary god of medicine. We have already seen in the "Philopseudes" how he was regarded as the representative of orthodox medicine as distinct from quackery, and it was undoubtedly from a desire to establish his claims as a healer of all diseases that Alexander professed to be his prophet. On the other hand, Asklepius, like the other Greek divinities, has his full share of Lucian's ridicule, as is seen from the wilful anthropomorphism with which he is portrayed in several of the satirical pieces. Thus in "Bis accusatus," 1, like all the other gods in Olympus, he is represented as having too much to do, and quite worn out by the worries of practice, by unpleasant sights, and the disagreeable things he has to touch. In the "Deorum Concilium," 6, Momus, while urging Zeus to expel some unworthy deities from Olympus, is forbidden to mention Asklepius, because "he cured the sick and was worth many men"—a quotation from the Iliad (xi, 514), which Lucian's Zeus seems to have had at his fingers' ends.

It would seem that Asklepius, like other deities, was not always content with the duties assigned him, for in a meeting of the divine assembly it was decided "that each god should mind his own business. Athene was not to treat the sick, Asklepius was not to prophesy, nor Apollo to do so many things at once. Each god was to choose one

[1] *Proceedings*, 1914-15, viii (Sect. Hist. Med.), pp. 49-58.

single occupation, and be either a prophet, a harpist, or a physician" (Deor. concil., 16; cf. Dial. deor., xxvi, 2). One of the most amusing "Dialogues of the Gods" is a dispute before Zeus between Herakles and Asklepius for precedence in Olympus (Dial. deor., xiii):—

Zeus: Herakles and Asklepius, stop quarrelling with one another, like mere men. For this is unseemly and unworthy of the banquet of the gods.

Herakles: But, Zeus, will you allow this druggist to take precedence of me at table?

Asklepius: Of course he will, for I am the better man.

Herakles: In what, you thunder-struck fellow? Is it because Zeus struck you dead for an unlawful act and now, out of pity, has granted you a share of immortality again?

Asklepius: What, Herakles, have you forgotten that you were burnt up in Oeta, that you should taunt me with being burnt?

Herakles: Our lives have been entirely different. I am the son of Zeus, and have accomplished so many labours by purging the earth, hunting down wild beasts, and punishing insolent men. But you are a herbalist and a quack; perhaps of some use for dressing the wounds of the sick, but with no manly achievement to boast of.

Asklepius: You speak truly, because I healed your burns when you came here half roasted, your body consumed first by the shirt of Nessus and then by fire. But, at all events, I was never a slave like you, nor was I beaten by Omphale with a purple slipper, nor did I kill my wife and children in a fit of madness.

Herakles: If you don't stop insulting me you will soon learn that your immortality won't help you, for I will take you up and throw you headlong out of heaven, so that even Paean can't cure you when your skull has been crushed.

Zeus: Stop, I say, and don't cause disorder in our assembly, or I will expel both of you from the banquet. But it is right, Herakles, that Asklepius should sit above you at the table, for he died before you did.

In striking contrast with these anthropomorphic conceptions of Asklepius is the following quotation from the "Demonax," a piece in which are chronicled the remarkable sayings of an Athenian philosopher of that name. One of the friends of Demonax says to him, "Let us go to the temple of Asklepius to pray for my son." Demonax thereupon replies, "Do you think he is deaf if he can't hear us praying where we are?" (Demonax, 27). Three other references are made to the Temple of Asklepius at Athens; two concerning the theft of a cup therefrom (Hermotimus, 37; Icaromenippus, 16) and one in relation to its being a haunt of the pseudo-philosophers who infested the city (Piscator, 42). In "Icaromenippus," 24, mention is also made of the Temple of Asklepius at Pergamum.

Apart from the worship of Asklepius, the relation between medicine and religion is illustrated in Lucian by the description of the semi-divine curative power attributed to the statues of departed athletes, soldiers, and other illustrious men. Thus the statue of Toxaris, the Scythian physician, who had helped to check the plague at Athens, was accredited with the power of having cured several persons of fever (Scytha, 2). The statues of the athletes Polydamas in Olympia and of Theagenes in Thrace, enjoyed a similar reputation (Deor. concil., 12) to which Pausanias (vi, 11, 3) also testifies. In the "Philopseudes," 19, Eucrates relates that the statue of Pelichus, a Corinthian general, cured a man of ague in three days, and as a reward was decorated with ribbons and crowns, its chest covered with leaves of gold, and pieces of money implanted in wax were stuck on it by grateful patients (cf. also De morte Peregrini, 28).

Prevalent Diseases.

Lucian's description of contemporary social life, especially in imperial Rome, as revealed in "Nigrinus" and "De mercede conductis," contains many indications of the diseases prevalent. No one familiar with the satires of Horace and Juvenal will be surprised to learn that the effects of gluttony kept the doctors busy in Rome (Nigrinus, 22). In "Gallus," 9, the cock consoles the cobbler Micyllus with the exclamation, "What diseases does not intemperance cause the rich! Gout and consumption, pneumonia and dropsy. These are the results of their costly banquets." Those who suffered almost as much as the rich themselves were their unfortunate dependants, who were compelled to keep late hours and to eat and drink to excess, with the result that they contracted dyspepsia, phthisis, pneumonia, rheumatism, or "a fine attack of gout" (De mercede conductis, 18, 19, 24, 31, 39; Cronosolon, 22). In several passages (Charon, 17; Abdicatus, 27; Gallus, 23; Epist. Saturn., 28) we find the collocation of the terms *φθόαι* and *περιπνευμονίαι*.

Alcoholism.

The association of pneumonia and phthisis with dropsy or gout suggests that alcoholism was the connecting link. Of the prevalence of alcoholism we have in Lucian, as in the Greek Anthology, plentiful evidence. In Rome it was especially rampant (Nigrinus, 15; De mercede conductis, 24). Many passages testify to the unpopularity of

the water-drinker or of the man who kept sober at a feast. An epigram in the Greek Anthology (Anth. Pal., xi, 429), sometimes attributed to Lucian, runs as follows: "When all were drunk, Akindynus insisted on being sober, therefore he alone of the company was thought drunk." Probably one of the reasons for the general dislike of the Cynics was that they drank water, "just like animals" (Cynicus, 5). It is amusing, therefore, to read that Diogenes in the Isles of the Blessed had so much changed in disposition that he was frequently drunk and danced, and had married Lais the courtesan (Vera Historia, ii, 18).

The danger of abstinence during a festival is shown by the case of the Platonic philosopher Demetrius (De Cal. non tem. cred., 16), who was accused before Ptolemy of drinking water during the Bacchanalia. He only saved his life by being drunk from an early hour in the morning.

Total abstainers were cursed with good memories, and would relate stories the reverse of flattering of their boon companions of the previous night. Hence arose the proverb—*μισῶ μνήμονα συμπόταν* (I hate a fellow-guest with a long memory), which Lycinus, one of the characters in the "Convivium," 3, quotes in relation to the surgeon Dionicus, who had been very useful in dressing the wounds inflicted by the philosophers on one another during the banquet, but had violated professional secrecy in divulging the details of the orgy the next day.

Alcoholic amnesia was not unknown to the ancients, as the following quotation from the "Charidemus," 2, shows: "It is not easy to tell you everything that happened at the feast . . . you know how wine makes men forgetful, even those with the best memory." Similarly in "De Dea Syria," 22, we read, "When wine is admitted anywhere, permission to say anything accompanies it. Refusal of a request is not humiliating, and everything that is said is soon forgotten."

From the piece entitled "Demosthenis encomium," 15, we learn that the celebrated orator was a teetotaler: "Unlike Æschylus who, according to Callisthenes, wrote his tragedies in wine, Demosthenes did not compose in wine but in water. For this reason Demades mockingly said that 'other orators spoke by the water-clock, but that Demosthenes wrote in water.'" This to us somewhat frigid jest seems to have been a favourite one in the law courts, for in "De mercede conductis," 35, a bibulous orator is described as measuring the length of his discourse not by the water-clock but by a jar of wine.

In Lucian, as in the Greek Anthology, many of the philosophers of the day served as models of intemperance. Several passages (Hermotimus, 11; Convivium, *passim;* Fugitivi, 19; Nigrinus, 25) show the

striking variance between their precepts of sobriety and their practice. The behaviour of Thrasyllus, as related by Timon, may be quoted as an example (Timon, 55): "In the morning he makes a thousand noble utterances about virtue, condemns men who delight in pleasure and praises frugality, but in the evening, when he has had his bath, and has come to the banquet, and his slave has given him a large cup of wine—he likes it best undiluted—then as though he had drunk of the water of Lethe his conduct is just the opposite of what he had preached in the morning. When he is drunk he does not restrict himself to singing and dancing, but indulges in abuse and anger as well. Cup in hand, he harangues the guests about temperance and orderliness, then overcome by the unmixed wine he stutters in a ridiculous manner and finally vomits. As he is being taken out, he seizes a flute girl with both hands." Drinking of unmixed wine was a barbarism of which the gods were also guilty. In the "Deorum concil.," 5, the right of Dionysus to rank among the gods is disputed on the ground that, in addition to not being Greek on his mother's side, he smelt of unmixed wine from an early hour in the morning. Cronos (Saturn., 2) is another deity remarkable for his alcoholic propensities, which explains his being, like ordinary mortals, a victim of gout (*ibid.*, 7).

The association of alcoholism with prostitution, to which I drew attention in the Greek Anthology, is well illustrated in the "Dialogues of Courtesans"—especially vi, 3; ix, 4; xi, xii, 1—in which the scenes take place in drinking taverns. It should be noted, however, that a young courtesan is warned by her mother not to get drunk when invited to dinner, "for such behaviour is ridiculous, and men detest such women" (*ibid.*, vi, 3).

Syphilis.

In this connexion it may be stated that the absence of anything suggestive of syphilis in Lucian's writings is extremely valuable negative evidence of the existence of that disease in classical antiquity. As in the Greek Anthology, so in Lucian there is the same frank presentation of the physical side of normal and aberrant sexuality which forbids translation into any modern language. Had syphilis been in existence at the time, Lucian would undoubtedly have presented one or more of his amorous gods or philosophers as victims of the disease, just as Voltaire, his modern counterpart, has done in the case of the optimist philosopher, Dr. Pangloss, in "Candide." But neither in the "Dialogues of Courtesans," the "Amores," the "Asinus," or the

"Adversum indoctum," to mention the most likely pieces, nor in any of the others is there any passage which could be tortured by an ingenious commentator to bear any reference to syphilis or indeed any venereal infection.

Gout.

It is undoubtedly from the burlesques "Tragodopodagra" and "Okypus" that Lucian is best known to the medical reader. In the former the chief dramatis personæ are a victim of the gout, a chorus of fellow sufferers, the all-powerful goddess Podagra, and two quack doctors. The scene opens with the lamentations of the gouty man, and continues with the chorus celebrating the mysteries of the goddess. Podagra herself then appears, and in her monologue enumerates all the remedies which have been tried and found ineffective.

The following list forms an amusing confirmation of the dictum of Aretæus, who was probably a contemporary of Lucian, that "the remedies for the disease are innumerable, for the calamity renders the patients themselves expert druggists" (Morb. chron., ii, 12).

It will be remembered that Sydenham, with grim irony, quotes this monologue in his well-known treatise on gout to forestall the reproach of being "either deficient in sagacity or niggardly in his list of remedies" (Essay on Gout, 70): "Ever since the human race was born all have sought to overthrow my power. Some have recourse to one method, some to another. They use plantain, parsley, lettuce leaves, meadow purslain, horehound, pound weed, nettles, comfrey, marsh lentils, carrots, leaves from the peach tree, hyoscyamus, poppy, onions, pomegranate peel, flea-wort, frankincense, hellebore root, fenugreek in wine, frog's spawn, lentil soup, cypress gum, barley flour, cooked cabbage leaves, pickled lobster, mountain goats' dung, human excrement, bean meal, and the flower of the stone of Assius. They cook toads and field mice, lizards, cats, frogs, hyænas, stags, and foxes. What metal, what juice, the sap from what tree have mortals not tried? All animals' bones, nerves, tendons, fat and blood, urine, dung, and milk —they have tried all."

It may be mentioned that eleven of the substances in this list are also recommended by Rufus of Ephesus, who probably lived under Trajan in the century before Lucian, in his treatise on gout—viz., poppy, hyoscyamus, plantain, flea-wort, fenugreek, lentils, cypress, onions, hellebore, goat's dung and pickle; and sixteen by Paulus Aegineta (Lib. 111, lxxviii), viz., hyoscyamus, purslain, marsh lentil,

poppy, flea-wort, mallow, lettuce, hellebore, cabbage, fenugreek, pomegranate, parsley, goat's dung, hyænas, and the flower of Assian stone (vide Adams's translation, i, pp. 657-676); and, as late as 1696, we find that human urine, blood, and bones, frog's spawn, deer's blood, dung and backbone, hyæna's dung and backbone, fox's oil and skin, are recommended by Salmon for the gout (The New London Dispensatory, fifth edition, 1696).

After the chorus have sung Podagra's praises, a messenger appears heralding the arrival of two doctors from Syria, who boasted that they had an ointment to cure gout. Podagra then summons her torturers to rack the joints of the gouty man, and bids the doctors apply their ointment. Needless to say it proves quite ineffectual, and Podagra triumphantly exclaims: "Let all know that I alone of the gods am inexorable, and yield to no remedy." The piece ends with a parody of the usual termination of a Greek tragedy: "Let every gouty man submit to mockery and ridicule, for such is his fate."

In the other burlesque, the authenticity of which is regarded as even more doubtful than that of "Tragodopodagra," an athlete named Okypus (Swift-foot), who had been in the habit of jeering the gouty, is himself attacked. Imagining that he had strained his foot while running or wrestling, he at first makes light of his infirmity, but as his suffering increases he is forced to summon the doctor, Soter, who reveals the unwelcome truth and forms a very gloomy prognosis. "I will not," said he, "have recourse to the deceitful words which most doctors use, but who know of no real cure. I will tell you shortly that you are suffering from a terrible disease from which there is no escape. Not only are your feet fettered in bonds of iron, a torture invented for wrong-doers, but your whole body will suffer a fearful, hidden and intolerable woe." The unhappy Okypus is then carried groaning off the stage and the play ends.

Many other references to gout are to be found in Lucian, some of which I have already quoted. Not even the gods were exempt, for Cronos (Saturn., 7) speaks of himself as an old and gouty man, and thus gives a rationalistic explanation of the legend that he had been chained up by Zeus.

Malaria and other Febrile Disorders.

Numerous passages in Lucian, of which some examples have been already quoted, testify to the prevalence of febrile diseases. The terms used are πυρετός and ἠπιαλός, the Attic term for ague. The exact

nature of these diseases is an interesting subject for speculation. Mr. W. H. S. Jones, in his valuable monograph on "Malaria and Greek History," has made out a strong case for malaria, but it is more probable that more than one disease was in question. The antiquity of typhus fever, with which the Plague of Athens has been identified by some medical historians, and the points of resemblance of some of the cases recorded by Hippocrates in his "Epidemics" to typhoid fever, relapsing fever, and Malta fever, should make us hesitate before adopting too exclusive a view. Especially interesting is the remark made by the speaker in "Abdicatus," 27: "To suppose that every fever, consumption, pneumonia, or insanity is the same in character and in every individual, is the sign of those who are not prudent or thoughtful, and who have not studied such subjects."

We have already seen that fever was ranked—with other diseases such as pneumonia, phthisis, and gout—as among the evils to which the rich and their dependants were especially liable. In the "Gallus," 9, the poor man prays the gods to visit with ague the guest on whose absence from dinner his own invitation depends. In "Jupiter Tragœdus," 25, Zeus admits that it is the Fates and not himself who decide whether a man shall die by lightning, fever, or consumption. All classes of society were liable to be attacked. "Is it not humiliating," exclaims Lycinus, one of the speakers in the "Navigium," 40, "that you should fall ill just like the common people and that fever should not recognise you as a king?"

The febrile attack might be mild, and be shaken off in a few days, especially when the subject was a hardy man of plain living like the cobbler Micyllus (Gallus, 23). The essay "On how to write History" opens with a fantastic account of a seven days' fever which attacked the inhabitants of Abdera in the reign of Lysimachus. All seem to have recovered, but while it lasted the patients were continually making tragic gestures and declaiming iambic verses. The epidemic was attributed to Archelaus, the tragedian having played the Andromeda of Euripides in the summer, when many of the spectators caught the fever on leaving the theatre.[1] A similar febrile attack following a chill which may well have been pneumonia is alluded to in the following passage: "I can foretell without having recourse to Castalia or to the Delphic tripod, that if anyone walks abroad naked when it is cold, especially

[1] Sir W. Osler has suggested to me that this epidemic was closely allied to the "dancing mania" of the Middle Ages.

if it is raining or hailing, he will catch a severe fever, and what is more, he will feel a great heat" (Disput. c. Hesiodo, 8).

In two passages we find fever given as one of the causes of death. Hermes, addressing Charon, in the dialogue of that name (17), says: "The messengers of death are many; agues and fevers, consumption and pneumonia, the sword, brigands, hemlock, judges and tyrants"; and in the "Cataplus," 2, Clotho, as she surveys the crowd of departed spirits brought by Hermes to the infernal regions, points to those who have died of fever, including the physician Agathocles. In the "De morte Peregrini," 44, the cynic philosopher Peregrinus had so violent an attack of fever after excess at table, that the physician Alexander told him that if he wanted to die, death had come of its own accord and there was no need to burn himself as he had intended. In another two passages we meet with the words ἐκ νόσου μακρᾶς (after a long illness), which may refer to the protracted course of malaria or some continued fever. Thus in "Cal. non tem. cred.," 5, Envy is represented in the likeness of "a pale, misshapen man with a piercing look like those who have been dried up by a long illness," and in the "Pro imaginibus," 5, we read of a woman who, as the result of a long illness, had very little hair left on her head.

Old Age and Longevity.

In his "Medica Sacra," or "Commentary on the Diseases mentioned in Scripture," the celebrated Dr. Mead has included a chapter on the "Disease of Old Age," which he devotes to the interpretation of the well-known chapter in Ecclesiastes. The example set by Mead is my excuse for drawing attention at this point to Lucian's remarks on the subject. Lucian, who himself lived to an advanced age, has left us several satirical pictures of old men. Of these we may mention in particular the description of the centenarian who, on arriving in Hades, relates how in the world above, though old, bald, blear-eyed, and snivelling, he had been constantly courted by numerous male and female admirers in expectation of a place in his will (Dial. Inf., ix). A similar picture is presented us in the following dialogue, in which Pluto listens to the complaints of a young man named Terpsion, "dead ere his time":—

Terpsion: Is it just, Pluto, that I should die at the age of 30, while Thucritus, who is more than 90, is still alive?

Pluto: It is indeed, Terpsion, since he never prayed for the death of his friends while he was alive, whereas you, in the expectation of his inheritance, were constantly plotting against him.

Terpsion: For ought not an old man who is no longer able to make use of his wealth, take leave of life and step out of the way of the young men?

Pluto: That is a new law of your own, Terpsion, that one who can no longer use his wealth for procuring himself pleasure should die. But fate and Nature have arranged things differently.

Terpsion: Well, it is just this arrangement of which I complain. Things should be done in their proper order. The oldest man should die first, and after him the next in age, but the contrary should never happen. A very old man should not be allowed to live, with only three teeth left, almost blind, needing four servants to support him, his nose full of mucus and his eyes of discharge, insensible to any pleasure, a living tomb, while death carries off the handsomest and most vigorous young men (Dial. Inf., vi).

The sense deprivation of senility is alluded to again in the "Cataplus," 5, where Clotho exclaims that it is useless calling to the men over 60, for old age had rendered them deaf.

The treatise on longevity is generally regarded as spurious, though included in most editions, as its language suggests a period much later than that of Lucian. It is, however, says Harmon, the American translator, "valuable as a document and not uninteresting in spots." The piece is little more than a list of remarkable examples of longevity, among which are included:—

Twenty-seven kings, aged from 80 to 150.
Seventeen philosophers, aged from 81 to 105.
Eight historians, aged from 82 to 124.
Seven poets, aged from 85 to 97.
Four orators, aged from 82 to 108.
One grammarian, aged 82; and
One law-giver, aged 85.

Medical men are not represented.

The writer remarks that longevity has been variously attributed to climate, soil, or diet, or to a combination of these causes. Philosophers and literary men, he says, provided they do not neglect their health, reach an advanced old age. The longevity of certain races is ascribed to more fanciful causes. Thus the old age of the Seres—i.e., Chinese—is attributed to their drinking large quantities of water. "Is it not probable," says Hufeland, in his "Art of Prolonging Life," commenting on this passage, "that they may even at that time have been acquainted with tea?" It is also stated that the Chaldæans lived over a hundred years, owing to their use of barley bread, as this was a cause of keen sight and of making the other senses unusually active. Some of the modes of death of the aged may be ranked among the curiosities of

medicine similar to those found in the Greek Anthology. Such was the death of Sophokles, choked by a grape-seed, and of the comic poet Philemon, who literally died of laughter on seeing his ass eat figs. It is solemnly asserted that the soldier and historian Hieronymus remained sexually potent until his death at 104. Finally, we may note that five died of voluntary starvation, of whom one, the philosopher Cleanthes, had a growth on his upper lip.

Physical Education.

Two dialogues which were probably composed by Lucian during the period between his rhetorical and satirical writings are of interest from the standpoint of physical culture. These are the "Anacharsis" and the "De Saltatione." The former is a dialogue between Anacharsis, a Scythian traveller, and Solon, the Athenian law-giver, who endeavours to defend the Greek practice of wrestling, boxing, disk throwing, and jumping against the foreigner, to whom such exercises appear a ridiculous waste of time. Readers of Galen know in what low esteem he held athletes, and it is not improbable that the sarcastic criticism of Anacharsis was the outcome of Galen's teaching (*vide* Galen, "Adhort. ad art. addisc.," xi, xiii, xiv, and "Ad Thrasyllum," 41).

In the "De Saltatione" Lucian seeks to acquit pantomime, for so the term ὄρχησις is best rendered, of the charge of effeminacy and indecency brought against it by the cynic philosopher Craton, and shows that it has a high value both mentally and physically. The superiority of the profession of the pantomime performer to that of the athlete is set forth as follows (71): "All the other professions promise either pleasure or utility, but pantomime possesses both these advantages. How much more pleasant it is to witness this than to see young men boxing and streaming with blood and wrestling in the dust, although pantomime often represents these movements in a safer, more graceful, and more pleasing manner. Besides the continual movements of dancing, the pirouetting, the leaps and falls on the back are both amusing to the spectators and wholesome to those who take part in the exercises." "Consider," says Lucian, "the universality of the art. It sharpens the mind, trains the body, and delights the spectators" (*ibid.*, 72). The physical requirements of the performer are thus described (*ibid.*, 74): "He should not be too tall, nor mean and dwarf-like, but well-proportioned. He should not be too fat, for that would be unconvincing, nor excessively slender, for then he would look like a

skeleton or a corpse. His body should be supple and firm, so that he may twist about if necessary or take a firm stand."

Lucian's enthusiastic defence of pantomime can only be appreciated when we realise that the theatre at Athens had sunk into decay, and that Lucian could not sympathise with the Roman enjoyment of gladiatorial shows (Preller, in Pauly's "Real-Encyclopädie der classischen Alterthumswissenschaft," 1846, s.v. Lucianus).

Baths.

Numerous passages in Lucian, as in the Greek Anthology, relate to baths, which played so important a part in the hygiene of the ancient world. A short piece, entitled "Balneum," or "Hippias," after its architect, contains a detailed account of a public bath, which is described as possessing all the perfections of which a bath is capable —viz., "utility, beauty, brightness, and elegant proportions." It seems to have consisted of six principal rooms: (1) a cloak room; (2) an attendants' room; (3) a room containing three cold water baths, and adorned with two statues of white marble, one representing Asklepius and the other his daughter Hygieia; (4) a room with a tepid bath; (5) a rubbing-down room for those coming from the palaestra; (6) a room with three hot baths. The building was also provided with two latrines, a water-clock, and a sun-dial. Lucian thus concludes his eulogy of the bath and its architect: "He who could see the building and not give it its proper praise would be, in my opinion, not only a fool but also an ungrateful or rather an envious man."

Other allusions to baths apply to the practice of taking a bath before a meal (De mercede conductis, 14; Cronosolon, 17, 23; Asinus, 2; Navigium, 22); to the use of baths by courtesans (Dial. meretr., vi, 4; Amores, 42); to the insolent display of the rich, who attended the public baths with a large retinue of slaves (Nigrinus, 34); and to bath attendants who soused the bathers (Demosthenis encomium, 16), or overheated the bath (Demonax, 42).

Cosmetics.

The references to cosmetics are all of a satirical character, and deal with face paints and dyes, especially those used by courtesans (Piscator, 12; Quomodo. hist. sit conscrib., 8; Dial. meretr., xi, 3), and pathics (Demonax, 50; Adversus indoctum, 23), the wearing of false hair (Dial. meretr., xi, 3; Alexander, 3, 59), hair dyes (Dial. meretr., xi, 3) and

epilation by the use of mastic (Adversus indoctum, 23; Demonax, 50; Pseudologus, 13). The following quotation may serve as an example in which the morning toilet of fashionable women is portrayed: "Countless different drugs are used to doctor their wretched faces, for it is not a stream of pure water into which they plunge their faces to wash off their deep sleep, nor do they set about any useful occupation, but they mix together numerous pastes to give a lustre to their disagreeable skin. As in a public procession each servant has some special duty to perform, one carrying a silver basin, another a jug, and a third a mirror. The room is filled like a chemist's shop, with boxes containing numerous deceitful compounds, some to clean the teeth and others to darken the eyebrows. Most of their time is spent in dressing their hair. Some dye their hair like wool with colours as bright as the sun at noonday. Those who think dark hair becomes them best spend their husbands' money in scenting themselves with Arabian perfumes. They shamelessly paint their cheeks so as to animate, by bright colours, the excessive pallor of their skin" (Amores, 39-41).

SUMMARY.

(1) Numerous passages in Lucian show that the medical profession during the second century A.D. enjoyed a high reputation.

(2) In addition to regular practitioners, however, charlatans drove a flourishing trade. Superstition, especially in therapeutics, was rife, and of this amusing examples are given both in "Philopseudes" and "Alexander."

(3) The worship of Asklepius was still in force, but Lucian's satire did not spare him any more than the other inhabitants of Olympus.

(4) The pictures of social life contain many allusions to prevalent diseases, especially pneumonia, consumption, alcoholism, gout, and fevers, which latter include malaria, typhoid, typhus, and Malta fevers, as well as to other subjects of medical interest, such as physical exercise, baths, cosmetics, and longevity.

(5) There is no evidence suggestive of syphilis in Lucian in spite of the large erotic element in his works.

The PRESIDENT (Dr. Norman Moore): I congratulate Dr. Rolleston on a most interesting and laborious piece of work. Lucian not only sheds light on disease and on the practice of medicine but also on the actual conditions of life of his time. A similar study of the life of Galen would bring forth many details and points of interest concerning the habits and customs of Roman citizens in the second century A.D.

Section of the History of Medicine.

March 17, 1915.

Dr. NORMAN MOORE, President of the Section, in the Chair.

William Withering, M.D., F.R.S.

By A. R. CUSHNY, M.D.

AT the end of the eighteenth century there was in this country a group of physicians which has never been excelled, perhaps never been rivalled, at any time here or elsewhere. Some of them have received their meed of fame, the names of others are perpetuated by being attached to various preparations of the Materia Medica, but of some of them the ordinary medical man knows nothing. It is curious to observe how little we, of the medical profession, recognise those to whom we are indebted for the progress of therapeutics. A surgeon may attach his name to an operation, an anatomist to some rare abnormality; but the discoverer of a valuable remedy has in the past generally been forgotten, and not only the discoverer but the method of discovery is lost in oblivion. How few recognise that all the remedies of the last half century have entered medicine through a single portal—that of the laboratory. Of such a forgotten discoverer in medicine I have to speak to-day—forgotten by his fellow-physicians only—for Withering's fame as a botanist is maintained by the genus *Witheringia* named after him, as his labours as a chemist are recalled by the *Witherite* which he was the first to examine and describe.

William Withering was born in 1741, at Wellington, in Shropshire, about 50 miles from Berkeley, where Jenner was born eight years later. His father, Edmund Withering, was an apothecary in Wellington, and his mother's brother was a physician in Lichfield, so that William

early came in touch with his future profession. He was educated at home, and by the Rev. Henry Wood, of Ercall, who seems to have made a deep impression on his pupil. In 1762 Withering entered the University of Edinburgh, where he studied medicine and obtained the degree of M.D. in 1766. He seems to have worked steadily in Edinburgh and to have assimilated the local method of profuse note-taking, which still plays such an important part in education in the North. At the same time he took part in the lighter side of student life. He was a Freemason and was, I suppose, one of the earliest Englishmen to appreciate the game of golf. In this connexion may also be mentioned that he attended the meeting of the General Assembly of the Kirk of Scotland. With an equal catholicity of taste, he seems to have shown some appreciation of the Scottish national musical instrument, and to have been himself no mean performer on the German flute. Some poetical efforts were published anonymously in the fashion of the time, but do not appear to have possessed great merit even in the eyes of his son and biographer. Withering was an enthusiastic supporter of the Medical Society which flourished among the students, and had also a wide social circle in Edinburgh. Among his friends in Edinburgh were a number of men who afterwards attained some distinction in life and continued to be his friends to the end. Among these were Fowler, who did much to introduce arsenic into therapeutics and whose name is associated with the arsenical solution; Bostock, the father of the well-known physician; Arnold, later a writer on insanity; Rogerson, physician to Catherine of Russia; and Percival, later a reformer in sanitation and in educational progress in Manchester.

The Principal of Edinburgh University at that time was the historian Robertson, and among the brilliant circle of Edinburgh was another historian of a different type—David Hume. The medical professors were: Hope in botany, an enthusiastic follower of Linnæus, who named a genus after him; Whytt in medicine, an authority on hysteria; Monro *secundus* in anatomy, who during Withering's stay in Edinburgh discovered the foramen of Monro (1764). But the man who apparently influenced Withering most in his subsequent career was Cullen, with whom he appears to have been on terms of intimacy. Cullen came to Edinburgh as Professor of Chemistry in 1755, and commenced clinical lectures in the hospital two years later. In 1760-1761 he also lectured on materia medica, and these lectures were obviously well known, as they were pirated some years later. Withering studied chemistry

under Cullen and he retained his interest in this subject throughout life. He was also indebted to him for his outlook in clinical medicine, and we find him early following his teacher in opposition to the popular Brunonian theory, which he admired for its symmetry, but which he held was founded on error, and must soon follow similar theories of medicine into oblivion.

Withering returned to Wellington during the long vacations, generally riding on horseback and visiting friends on the way, or deviating from the direct route to see some of the interesting places in the North of England. Once at least, and possibly twice, he travelled south by the London packet, and spent some time in the London hospitals. It is interesting to find the opinion expressed in 1764, that in the Scotch schools systematic teaching was carried to an extreme, while in London too little attention was paid to it. This was in 1764, but I seem to have heard the same double-edged criticism one hundred and fifty years later.

Withering graduated M.D. in 1766, his thesis being "De Angina Gangrenosa." After a short stay in London and a visit to Oxford he joined a Mr. Townshend in a tour of France and Paris. This does not seem to have been very successful, as though he enjoyed the beauty and art treasures of Paris, he seems to have had some difficulty with the language and to have resented the arbitrary character of the Government. His companion died in November, and Withering had some difficulties with the legal forms in use in the case of a heretic, and was glad to return home in December, 1766.

He began practice in Stafford in the following year and remained there till 1775. At Stafford he began to study the local flora and soon became an enthusiastic botanist: This does not appear to have arisen from his studies at Edinburgh under Hope; for these do not seem to have been attractive, as I find him writing in Edinburgh that the gold medal for botany "will hardly have charm enough to banish the disagreeable ideas I have formed of the study of botany." A more powerful influence was required, and this was found in the desire "to supply subjects for the highly finished drawings" of the young lady who became his wife a few years afterwards. I do not propose to follow Withering as a botanist. Suffice it to say that he successfully modified the Linnæan system in certain points and wrote a "British Botany" published in 1776. A new and enlarged edition was published in 1787, a third volume added in 1792, and a third edition extending to four volumes in 1796. This work enjoyed a wide popularity in the first half of the

nineteenth century, during which a number of new editions were published by William Withering the younger, and it is said to be still in use by very conservative lovers of flowers. I can recommend it as much more entertaining than the modern business-like systematic text-books. The author gives not only the description of the plants, but their uses, and very often references made to them by the poets. As I am discussing an eighteenth century character, I may be allowed to say that in his botany Flora maintains her alliance with the Muses.

Withering was very popular in Stafford and the district, and was sole physician at the county infirmary, which owed much to his exertions, but the practice was a very poor one, only averaging about £100 a year at first. He therefore set up practice in Birmingham in 1775, as successor to Dr. Small, though he continued to visit Stafford Infirmary once a week for some time. In Birmingham his success was rapid, and very soon he was reputed to have the best practice out of London. He took up the study of chemistry, especially the analysis of natural waters, and translated and edited a treatise of Bergman's under the title "Outlines of Mineralogy," 1783. A number of chemical papers were also contributed to the *Philosophical Transactions of the Royal Society*, among them his work on "Terra Ponderosa," which has since been known as "Witherite," in his honour. The Royal Society elected him Fellow in 1784, the Linnæan in 1791.

At Birmingham Withering found a congenial circle in the Lunar Society, which met monthly at the full of the moon, and whose most eminent members at the time were Priestley, the great chemist, James Watt, of the steam engine, Boulton and Keir. The dominant note of this society was science in all its branches. We find Withering satisfying himself that the view prevalent then and now that the weather is influenced by the phases of the moon is quite inconsistent with facts; at another time he is examining the fossil bones of an extinct tiger. He spends some time in "combating that monster Phlogiston," but finds that it would occupy more time than he can spare from practice, and hands over the subject to Priestley, whose results laid the foundation of modern chemistry.

The fascinations of scientific research did not distract him from medicine, however. In 1778 he published his "Scarlatina Anginosa," and the same year the Birmingham General Hospital was completed, largely by his efforts. In 1785 he published the little book which has the greatest interest to his profession, and which I think more than either his botanical or chemical work entitles him to lasting fame.

This book is entitled "An Account of the Foxglove and some of its Medical Uses; with Practical Remarks on Dropsy and other Diseases." Up to this time digitalis is mentioned occasionally in medical literature, and it enjoyed a more or less doubtful position in the treatment of phthisis, and as an external application in scrofula, &c. After Withering its action in dropsy and on the heart is generally recognised, and though one finds the foxglove put to strange uses in the earlier part of the nineteenth century, yet its main effect has never again been lost sight of since it was introduced in 1785.

Withering states that his attention was drawn to digitalis in 1775 by finding that it was an ingredient in a cure for dropsy which had long been used by an old woman in Shropshire. You may remember that Jenner's attention was first directed to vaccination by a young country woman. Foxglove was accordingly prescribed in Withering's dispensary, and apparently his interest was further stimulated by hearing that it had been employed successfully in the case of the Principal of Brasenose College, Oxford. At first the leaves were made into a decoction, later into an infusion, and sometimes they were used as a powder. The doses prescribed were very large, for example, the decoction from 9 gr. of the leaves every two hours, until vomiting followed. Our British Pharmacopœia suggests as a maximal dose the decoction from ½ gr. These doses proved a sovereign remedy in the numerous cases of dropsy which he gives. An equal success was sometimes obtained by smaller doses at longer intervals, which did not disturb the stomach at all. He notes that the cure had often to be repeated several times at intervals, and that in these subsequent treatments a much smaller quantity sufficed than at first.

The use of digitalis quickly spread among Withering's friends in the profession and especially in Edinburgh, where Hope was its sponsor, and in 1783 it made its first official appearance in the Edinburgh Pharmacopœia. It is interesting to meet the name of Charles Darwin as recommending its use on the ground that it had proved valuable in the hands of his father, Erasmus; this Charles Darwin was, of course, the uncle of the great naturalist.

After ten years' trial of this new remedy, Withering was induced, by the urgent desire of his friends, to write down his experience with it, and the booklet bears the motto, *nonumque prematur in annum*. The deliberation with which the discovery was brought before the world gives a delightful impression of the easy life of the eighteenth century. Now we find a new remedy exploited after an experience of months,

sometimes only of weeks. Perhaps a return to the older and more cautious method is too much to wish for, but I doubt if therapeutics would suffer from it. Withering's book contains 163 cases in which he had used the remedy himself and a number of others in which his friends had used it at his suggestion. He sums up his experiences at the end in a few pages, and one paragraph, which is printed in italics, runs as follows: "*Let the medicine therefore be given in the doses and at the intervals mentioned above; let it be continued until it either acts on the kidneys, the stomach, the pulse, or the bowels; let it be stopped upon the first appearance of any one of these effects.*" A few years ago a very careful examination of the effects of digitalis was made by Mackenzie, and I think he would be the first to admit that Withering sums up in this paragraph the main principles of digitalis treatment. From the description he gives of them, a number of Withering's cases obviously suffered from auricular fibrillation.

Withering used digitalis only as a diuretic in dropsy and anasarca, and is careful to state that it is valueless in ovarian cysts and similar conditions. He realised that it was not a universal diuretic, though it was superior to any other medicine and often succeeded after all others failed. He did not appreciate how it acted in dropsy, nor does he differentiate the cardiac from other forms. At the same time, he seems to have seen dimly that it acted on the heart, for his last aphorism is, "That it has a power over the motion of the heart to a degree yet unobserved in any other medicine and that this power may be converted to salutary ends."

I have dwelt upon this booklet of Withering's because I regard it as much the most important work of his life. He was a great botanist, a good chemist and mineralogist. But a discovery in chemistry or in botany is not to be compared with an important advance in therapeutics. The difficulties attending the investigations are altogether disproportionate. The chemist works with materials of the same nature and changing accordingly to definite laws. But what law governs the changes in a sick man, or who shall judge whether the conditions are similar in two cases of the same disease? The difficulties in the problems of therapeutics are so overwhelming that one is overcome by astonishment that progress has been so great and so rapid in the past. Withering also seems to have recognised that this work on the foxglove was on a different plane from his other work, for we find that in his picture he is represented with a branch of the foxglove in his hand, and on his tomb this plant is also sculptured.

I need not dwell further upon the importance of the discovery of digitalis, but I cannot forbear giving you the following lines by "S. H.," which I find in a late edition of Withering's Botany:—

> The foxglove's leaves, with caution given,
> Another proof of favouring Heav'n
> Will happily display;
> The rapid pulse it can abate;
> The hectic flush can moderate
> And, blest by Him whose will is fate,
> May give a lengthen'd day.

Another medical publication of Withering's was "An Account of the Scarlet Fever and Sore Throat, or Scarlatina Anginosa, with Remarks on the Nature and Cure of the Disorder." This was first published in 1778 and a new and enlarged edition followed in 1793. It seems to have received much appreciation at the time, but has not retained its early reputation.

Withering had a large circle of friends in Birmingham and maintained constant connexion with many of his profession in London and throughout the country. His botanical work introduced him to many of the Continental scientists and he received cordial recognition at their hands. Many distinguished men of science are mentioned as having visited him in Birmingham, which appears to have attracted them from its rapid development of metal working. He seems to have taken only a mild interest in politics, and to have been a moderate Progressive, holding that the constitution under which George III held the crown required some modification, a view with which few will quarrel. In spite of this moderation, when the mob rose and burned Priestley's house in 1791, that of Withering was also attacked and he had to flee, carrying off his most precious books and herbaria in wagons covered up with straw. This unpopularity appears to have been only temporary and to have been altogether undeserved by any action on his part; he was not present at, and had in fact refused an invitation to the famous dinner which served as a pretext for the riot. Thus in 1797, when invasion of England seemed possible, we find him working for the war fund, and hoping that the universal contribution "will produce a good effect on our enemies and cannot have a bad one on our friends." Writing to a foreign correspondent, he says: "No, we are in a high state of unanimity and preparation, determined not to exchange sterling English liberty for French; because, though of late clipped a little, we

know it to be standard." The following may strike a sympathetic note to-day: "Scientific matters are partly at a stand with us, almost every active man being enrolled in some form or other of military duty. We no longer fear invasion, the spirit of resistance being as universal as it is animated . . . ."

Withering began to have threatenings of pulmonary phthisis about 1780, and in 1783 he had to retire from practice for some months; and this ill-health was one of the chief reasons for his going to live at Edgbaston Hall, a "spacious and commodious mansion surrounded by a park." In 1790-91 he had repeated attacks of pleurisy, and from this time his strength declined, and he was no longer able to devote himself to his practice, and had to resign from the infirmary. In 1792 he tried the effect of a warmer climate, spending the winter at Lisbon, which enjoyed some reputation as a winter resort at that time. He pursued his botanical work, and also analysed the waters of the springs at Caldas da Rainha, near Lisbon. This scene is represented (with some poetic licence) in the vignette attached to one of the engravings which I have here. Withering was not favourably impressed by Lisbon as a resort for phthisis, and obtained little benefit from it himself. He states very strongly that "Phthisis is infectious"; this may have been recognised by earlier writers on medicine, but sounds strangely modern from a writer of the eighteenth century. He repeated his visit to Lisbon in the winter of 1793-94, but had a bad return voyage, and appears to have benefited little. He recommends the South of England as at least equal to the Continental resorts for consumption, and advises especially the south shore of the Isle of Wight, but himself remained at Edgbaston in the winters afterwards. Here he had his library kept at a constant temperature of 65° F. by means of flues; the windows were double sashed, and every movement was avoided as far as possible. This treatment is an interesting contrast to the open-air methods of to-day. His chief occupation was the revision of his text-book on botany. which was completed in 1796. From this time onward his condition became worse, and he was confined more and more to the house, and was obviously sinking. One of his friends is credited with the remark, "The flower of English medicine is, indeed, Withering." On October 6, 1799, the end was approaching, and he took leave of his family, saying, "See how easily I lie"; he asked to be raised up in bed, and then ejaculating, "Now I am ready," fell back dead. He was buried in the old church at Edgbaston, which contains a monument inscribed with his name and

encircled with the Witheringia and with the purple foxglove of which he wrote. I forbear giving the poetical effusion attached to it, which is in the rather florid style of the period.

A picture of Withering was painted by C. F. von Breda in 1792, and passed to his son on his death, but I am unable to find where it is at present, or whether it still exists. The Director of the Birmingham Art Gallery writes me that he knows nothing of it.[1] Two engravings of this picture are extant, one in the collected papers issued by his son in 1822, the other in "Thornton's Sexual System of Linnæus," 1801. I obtained a separate copy of this engraving a few years ago, and have pleasure in showing it you. A short life of his father is prefixed to the Miscellaneous Tracts issued by William Withering, jun., in 1822. While making the necessary deductions from such filial biographies, one cannot but obtain a most charming picture of Withering. One can almost see him, gold-headed cane in hand, penetrating eyes, keen, intellectual face, as he visited the infirmary or went the round of his practice. He embodies the best type of the stately eighteenth century physician, still trammelled with the remains of theories which to us seem fanciful enough, still holding much to emetics and violent purgatives, still treating symptoms without due appreciation of the disease behind them, but tender and sympathetic to his patients, applying every faculty to relieve their pains, and "often, though not so often as he would, bringing healing." Again, we may see him at the Lunar Society, discussing and unravelling the somewhat nebulous chemical experiments of Priestley, reading a paper on Stonehenge and on the fall of the three great stones there, introducing some distinguished visitor to Birmingham, ever keenly interested in the advance of scientific discovery, yet not without a realisation of some of its humours (as is seen in the title of one of his papers, "On the Death and Burial of Phlogiston"). Above all his other interests, medicine was the mistress of his heart—chemist, botanist, antiquarian, he was first of all the loved physician.

[1] Sir William Osler informs me that it hangs in one of the Birmingham hospitals.

DISCUSSION.

The PRESIDENT expressed the thanks of the Section to Professor Cushny for his concise and lucid demonstration of Withering's pre-eminence as a contributor to the science of therapeutics. He (the President), when a clerk at St. Bartholomew's Hospital, had once witnessed the powerful effects of digitalis upon a ward cat. The animal drank deeply from a bowl containing an infusion of the drug in which flannels had been soaked and then applied to the abdomen of a patient suffering from dropsy. After drinking, the cat walked two-thirds of the way up the ward and then fell dead. It was curious that poisoners in days gone by were unacquainted with the deadly properties of digitalis. No instances are recorded of the criminal use of a poison so easily procured and administered, and so difficult to detect. Its value as a remedy was only recently appreciated. It was used in 1806 in the case of Charles James Fox, who at the age of 56 suffered from ascites, no doubt due to cirrhosis of the liver induced by drinking spirits. It was clear that his end was hastened thereby, and the world attributed his death solely to the use of this new remedy, which consequently fell into disrepute.

Sir WILLIAM OSLER pointed out that Withering had discovered the secret of the use of digitalis; and the indications which he relied upon for its employment did not differ much from those enumerated in the present day. The treatment of Withering himself—namely, by protection from exposure to cold and residence in a temperate climate during the winter—was not inappropriate for a patient in the third stage of pulmonary tuberculosis.

Dr. LEONARD GUTHRIE, alluding to Withering's statement that he learnt the use of digitalis from an old woman in Shropshire, said that readers of George Eliot would remember how Silas Marner cured a woman suffering from heart disease and dropsy with a simple preparation of foxglove after the doctor had done her no good. It would thus appear that country herbalists in George Eliot's time were more familiar with the virtues of the plant than medical practitioners, although nearly a century had elapsed since Withering described them.

Thomas Baynton, 1761-1820.

By J. A. Nixon, M.B.

The chance discovery of a contemporary biography of a surgeon who saw further ahead than many men of his own time is my reason for bringing to the notice of this Section the name of Thomas Baynton. I thought his name was quite unknown at the present day until I found in Von Bergmann's "Surgery" that the familiar method of strapping varicose ulcers of the leg by overlapping encircling strips of plaster was there described as Baynton's method. This contribution to the art of surgery might not by itself form a sufficient claim to be remembered by posterity. But the criticisms of Baynton's practice contained in the memoir with which this paper concludes led me to a further study of his writings. From these it is evident that the modern treatment of spinal caries was introduced by him. Baynton was the first surgeon (in England at any rate) to advocate the treatment of spinal caries by absolute rest in the horizontal position without the aid of caustic issues and setons. He led the revolt against the unscientific barbarities which were formerly practised under the guise and plea of counter-irritation. The leaders of surgery, particularly Earle, shocked at his temerity, opposed his views with vehemence. Nevertheless, Baynton was fortunate in numbering among his friends and supporters wiser and greater men—namely, Edward Jenner, to whom he dedicated his pamphlet on the "Treatment of Diseases of the Spine," and Anthony Fothergill, to whom he addressed his "New Method of Treating Old Ulcers of the Legs."

Baynton's advice on the treatment of diseases of the spine may be summed up in one or two quotations from his work on this subject. "My success," he says, "has been obtained by the application of simple principles, which are generally admitted in the treatment of some other diseases, and may have been acted upon in this, though neither myself nor the public have received the information." After referring to the use of "machinery for preventing pressure, as recommended by Sir James Earle," and "the use of drains occasioned by caustics, setons, &c., for the purpose of arresting the progress of disease in the bones, as advised by Mr. Pott," this not very eminent Bristol surgeon ventures to scrutinise their opinions and to ask "whether greater advantages

may not in future be expected from the use of means that are more simple, more reasonable, and much more agreeable." He argues that the repair of healthy bones when fractured is secured by fixation and rest, that ulcers of the legs are healed by resting, and proceeds to state that "a system of resting in the horizontal position regulated by scientific principles will accomplish the cures of diseases of the spine after the failure of drains and machinery, steadily continued a considerable number of years under the direction of skilful surgeons." Pott and Earle, he observes, made use of the horizontal position in curing diseased spines, even though they wrongly ascribed their success to setons, drains, and mechanical apparatus. It was Baynton's belief that drains are not generally, if they are ever, necessary for the curing of such diseases. "If so," he continues, "how much of that suffering which was experienced in the treatment by the making and dressing of setons, the application of caustics and the lying on open ulcers, of considerable extent, filled with pease, beans, or hard bodies, will be prevented by the adoption of this [i.e., Baynton's] plan of treatment.

"The following cases [he gives detailed accounts of thirteen] will clearly show that after very patient trials of partial resting, conjoined with the use of drains, and machinery, without any success, the most hopeless cases have been cured by uninterrupted [or as he elsewhere calls it 'undeviating, absolute'] rest alone." Baynton did not claim for himself priority of authorship for this plan of treatment. He says later, "With the exception of a French author, Mons. David, I have no recollection of any person who has suggested that rest would effect the cure of diseases of the spine." The description of the method of securing "undeviating, absolute rest" is as follows: "A crib or narrow bedstead must be procured, six feet in length, or rather of sufficient length to accommodate the patient; two feet one inch in height from the floor of the apartment to the floor of the crib, whereon the mattress is placed; two feet five inches wide, with posts three feet seven inches high (including castors) to be turned (by a turner) as a common crib. It must be provided with a rail floor, instead of sacking; and with side boards to raise up and down; which, when half raised, will resemble the raised flap of a table and must be supported with sliders, that can be drawn in or out when required; and which, when wholly raised, will furnish sides to the crib for security, or warmth at night." In this crib is to be placed a mattress which "should be as smooth as the seat of a sofa, perfectly flat, and nearly as hard as it can be rendered by the

skill of the manufacturer. The sides or flaps of the crib, when supported in their half-raised, or horizontal position, will answer the purpose of a table, and serve, when covered with blanket, to receive the patient once in a fortnight, or as often as may be necessary to turn the patient." In this crib the patient is to be horizontal for many months, enjoying fresh air and plenty of good nutritious food. The education of children is not to be neglected, whilst amusement and companions are to be encouraged. Indeed, Baynton himself apologises for devoting too much space to minutiæ. The cases he describes were successfully treated by prolonged rest over periods varying from six to eighteen months.

The opposition aroused by Baynton's views was violent, as may be gauged by such criticisms as Estlin's: "I conceive, that in the greatest number of cases, the observance of undeviating rest in the horizontal posture will be found a valuable addition to the present mode of treating diseases of the spine, and for this Mr. Baynton is entitled to the thanks of the profession; but I consider his object, of the exploding the use of issues in this disorder, as an attempt at a dangerous innovation, and one that demands the most decided opposition of his professional brethren."

Having thus allowed Baynton to tell in his own words the details of his "New Method of Treating Diseases of the Spine," I will leave it to his contemporary, Richard Smith, a famous surgeon at the Bristol Infirmary, to complete the story of the man and his life. Richard Smith's memoir of Baynton forms part of a vast manuscript collection of historical and biographical notes, comprising fourteen volumes, now in the possession of the Bristol Royal Infirmary, where Smith was for forty years an active member of the surgical staff up to the time of his death in 1847. This memoir begins with a notice of the death of Thomas Baynton's father, quoted from *Pine's Bristol Gazette*, March 9, 1775.

> Died, Mr. Baynton, Surgeon in Gloster Lane, commonly called Doctor Baynton, who was of very great practice, and whose loss will long be felt, especially by the poor.

The subject of this memoir was born on 5th Oct., 1761, near the foot of Clifton Down. I am alike ignorant of the nature of his education and where he received it. I do not think that he knew much of the later classics, and he certainly had no Greek. I am inclined also to think that he was never fond of the Belles Lettres, and, in fact, he troubled himself but little about literary pursuits.

His indenture is dated 5th Sept., 1775, he paid to the apothecary, Mr. Elmes, a gratuity of 20 guineas, and the same sum annually to the

charity during seven years. The day of his coming to the House was that of Mr. Godfrey Lowe's election. Mr. Thomas Godwyn, Surgeon, late of Redclift Hill . . . who was his fellow apprentice, told me that Mr. Baynton was such a tyrant that there was no enduring him, and that he used to beat Godwyn so that his friends removed him.

With the exception of Messrs. Pott and Bromfield, then two of the most eminent surgeons in London, I am at a loss to know what lectures he attended in the Metropolis. He began to practise at Bristol about 1782. On the 22nd of May, 1784, he married Miss Annie Swayne, of Hereford.

About Oct. in this year he received from the benevolent Dr. Fothergill[1] the honorary medal of the Humane Society for having restored a person "apparently dead by drowning."

In 1809 he went to reside at 20, Brunswick Square, and being now in easy circumstances he indulged himself in a pretty place at Hambrook, where he usually spent his Sundays and resided a good deal in the summer.

Almost at the very outset of his career he undertook the doctoring of four parishes in Kingswood, a business not in itself very profitable, but it led to better things. It brought him into contact with the Smith family of Stapleton, and also that of the Dowager Duchess of Beaufort. Mr., now Sir, John Smith, Bart., of Ashton Court, took a great liking to him and patronised him very much, and Mr. Baynton had on his part a persevering industry and good health, so that he very soon got into a very good line of business in the home districts of the County of Glocester (sic) . . . . he constantly appeared in dirty leather breeches and boots, with his clothes and hat in keeping, so that he seemed to have just quitted his horse after a hard ride into the country . . . a well-known trick.

His apprentice, Dew, said that his Master's House at this time was penury itself, and Baynton so close-fisted "that the pump-handle was troubled more at meal-times than the beer-cock."

[In 1783 he was an unsuccessful candidate for a Surgeon's vacancy at the Bristol Infirmary; he never again presented himself for election.] I believe upon the whole, although it hurt his feelings, it was much more advantageous to him to have remained out of the House. He avoided being implicated in the various cabals and altercations in which the officers of the Infirmary have invariably been engaged during the long period of forty years that I have known the Charity. However, he considered himself to have been overlooked, and certainly he retained to the last a hostile feeling towards the Faculty of the Infirmary, and seldom called in one of the Surgeons if he could avoid it. On more than one occasion he appeared in the ranks of opposition, and in particular when he headed a party in a great struggle to throw open the doors of the Operation Room to the practitioners of Bristol. He offered to the Trustees to raise amongst the Surgeons of the City a hundred guineas

[1] This refers to Dr. Anthony Fothergill, 1732-1813. Dr. John Fothergill had died in 1780.

annually provided in return that they might be present at the operations. The Society however negatived the proposition without a division.

[He took in 1816 a partner named Hill, who informed Richard Smith that in the most profitable year he ever had he booked £2,500, but there was nothing like that sum received.] Mr. Baynton refused no kind of midwifery case, and went to great numbers for a guinea. He continued to practise midwifery to the last, but he, a few years before his death, had the mischance at Clifton to lose four women of consequence within a short time and it had a very decided effect upon his practice in that line.

"A Descriptive Account of a New Method of treating old Ulcers of the Legs," published in 1707, and dedicated to Dr. A. Fothergill, F.R.S., laid the foundation stone of the great name which he acquired, the plan being adopted extensively both in the Army and Navy. The method consisted in the neat and regular application of adhesive strips to the limb and subsequently keeping the whole moistened with cold water. The principle, which is nothing more than affording a support to the parts, is as old as Wiseman. Mr. Baynton's master, Pott, of London, also strenuously recommended the laced stocking or the tight bandage, which are the same thing in effect, but the adhesive has the advantage of being cheaper and always at hand.

To this day (1830) I am not aware that any improvement has been made upon his suggestion, and the method having stood the test of 33 years' experience is a sufficient proof of its merits. Mr. Baynton, who either believed himself or wished to make others believe that his strips were omnipotent, used to assert that, in the generality of ulcers, he "could command them to get well . . . in most he could persuade them by degrees, and in some very obstinate ones he was obliged to use coaxing, but none dared to disobey ultimately." [Two persons whose cures appeared in Mr. Baynton's book were at that very time patients in the Infirmary for the identical ulcerated legs, says Richard Smith.]

In December, 1813, he published a pamphlet upon Diseases of the Spine in order to show that a mere horizontal position was sufficient for a cure unaided by caustic issues or setons. This doctrine was attacked with great severity in the medical periodicals, but chiefly by Mr. Earle, a surgeon in London. Neither was the dissent confined to the metropolis. In February, 1818, Mr. J. B. Estlin published a pamphlet in Bristol containing "Strictures upon Mr. Baynton's practice with the addition of cases to prove that gentleman to be in error." Mr. Estlin's paper was written in a mild and gentlemanly tone, and contained nothing that justified resentment on the part of Mr. Baynton. But the great success of his book on Ulcers had given to Mr. Baynton's mind a high opinion of the superiority of his own attainments, and he thought it little else than impertinence for anyone to dissent from his ipse dixit.

That he was well acquainted with the great principles of Surgery and Medicine there can be no doubt, but he was not backward in cherishing the idea that he had outstripped all his brethren in professional acquirements.

I never was in his company without hearing him relate some Wonderful Wonder in rescuing some patient from the jaws of death. If this ever meets the eye of any member of the Medical Book Society [of which B. and R. S. were members] I am sure that he will acknowledge the truth of this record and perhaps add that his whole conversation centered in himself and his profession.

He was exceedingly sanguine, catching eagerly at all new medicines or proposals, and, to use the language of a case he published, "flying to his patient with the remedy in his hand"; these *remedies*, as they were termed, had of course their day and then sank into oblivion.

Mr. Baynton had in the greatest possible perfection the art of "talking over patients." He usually began by alarming them: "He did not know what to say exactly—he feared there was great reason to apprehend the worst—it was almost too late to undertake the case—but it was one of those cases to which he had turned his particular attention—he had seen a great many—had several now under his care—and if anyone could cure it, he was happy to say —*he was the man*. He knew also the value of 'my good friend' and 'my dear Madam' even in the lowest classes, and turned all to advantage."

He avoided consultations, preferring to give up the patient rather than have another opinion. He considered that no one could know more than himself and that the proposal for further advice was rather affronting him.

In his early days I do not remember him, but I have often seen him operate in his late years, and I thought but very poorly of him. He made a great display and flutter, talked incessantly both to the patient and the assistant, but was tedious and indecisive in the stroke of the knife and not without tremulousness in the hand. In his person Mr. Baynton was rather handsome, being well grown, ruddy, and of a fair complexion. He had a very pleasing and gentlemanly address—he "boo'ed" somewhat too much, as Sir Archy McSycophant calls it, and endeavoured always to play the agreeable. His voice was remarkably musical and he had an oily smoothness and volubility of tongue which (with strangers especially) was particularly captivating. His great aim through life had been to accumulate a good round sum of money, and a fortunate purchase of a piece of ground at an ordinary price began to realise his wishes. Upon digging into this land coal was found, for the working of which he made a riskless and profitable bargain with a miner. His income from various sources was at the least £3,000 a year, and although he lived very comfortably yet he must have had a large surplus.

His monies at his decease amounted to about £33,000, and he had land also and considerable family expectances. He made his own will, the grand object of which was to build up hereafter in a lapse of years a Great Man, and it was so full of contrarieties, safeguards and postponements that his executors, not being able to comprehend his intentions, refused to act. So much of the bulk being tied up for an hereafter did not at all suit the views of his family, and a process was commenced to set aside these clauses. In this

difficulties have arisen, so that in all probability the gentlemen of the long robe will reap the benefits of Mr. Baynton's great and unremitting exertions to accumulate a large property.

Of his death Richard Smith writes :—

On the evening of the 29 of August, 1830, I happened to be at the Play—the lobby-keeper came to say that a gentleman's carriage was at the door for me, I learned that it was Mr. Baynton's. I found that he had retentio urinæ for 48 hours and was in great pain. I wished him to allow me to pass a catheter, this he peremptorily refused, alleging that he had been so once before and had then been relieved by Nature. We had therefore recourse to the bath and so forth. At 12 the next day I thought him dying, but about 7 in the evening he spontaneously emptied his bladder and seemed quite well again. "You see, my friend," said he, "I told you I should relieve myself." Everything seemed to promise well. But early in the morning he complained of his head, he soon became delirious, then insensible, and so departed this life. When he became worse almost every medical man in Clifton was sent for, the room was quite full of us, but all hope of the case was at an end upon the first glance at the patient.

A newspaper announcement of his death reads thus :—

Died. Thursday last at Clifton, Thomas Baynton, Esq^{r}, a surgeon not less celebrated for eminence and skill in his profession than he was distinguished for the feeling of humanity of his disposition and general amenity of his manners. His essay on the treatment of Ulcers will perpetuate his memory as one of the greatest alleviators of as painful a disease as any to which the human frame is incident.

He died as he had been born, in a house "at the foot of Clifton Down," next door to Duncan House, which still remains.

He achieved an unwonted reputation and success in his lifetime, and his method of strapping ulcers is in vogue to the present day, enshrined as already stated in Bergmann's great work on surgery as "Baynton's method." But his greater claim to the respect of posterity rests upon his strenuous though bitterly opposed plea for rest in treatment of spinal caries and the abandonment of the barbarous issues and setons which in his day were held to be the essentials of the cure. To the credit of the *Edinburgh Medical Journal*, one solitary voice hailed the heretical opinion as being at once sound surgery and common-sense.

BIBLIOGRAPHY.

BAYNTON, T. "Descriptive Account of a New Method of treating Old Ulcers of the Legs," Bristol, 1797.

Idem. "An Account of a Successful Method of treating Diseases of the Spine," Lond., 1813.

Edinburgh Medical Journal, 1814, x, p. 389, "Review of Baynton on Diseases of the Spine."

Ibid., 1815, xi, p. 35, "Reply to the Review of Mr. Baynton's Essay on the Cure of Crooked Spine," by Henry Earle.

ESTLIN, J. B. "Observations on Diseased Spine, illustrated by a Case, and containing Strictures upon Mr. Baynton's Treatise on that Disorder," Bristol, 1818.

BIOGRAPHICAL NOTES OF BAYNTON.

"Dict. Nat. Biography."

"Biog. Dict. of Living Authors," 1816, pp. 17, 412.

"Richard Smith MS." (Bristol Royal Infirmary).

DISCUSSION.

The PRESIDENT said that Baynton's work was an example of the stimulating effect exercised by Percival Pott upon his pupils, many of whom had dedicated their books to him. Pott was the first surgeon to be elected at St. Bartholomew's Hospital who was not at the time of his election a member of the Barber Surgeons' Company. Smith's description of Baynton's manner in dealing with patients reminded one of the surgeon in "Tom Jones" on the occasion when Jones sustained a broken head during a brawl in a tavern. Baynton, of course, lived a little too late to serve as Fielding's model. He (Dr. Moore) considered that the Section did good service in drawing attention to such men as Baynton, who would otherwise be forgotten.

Dr. PARKES WEBER remarked that Smith's sneer at Baynton's claim to have cured ulcers which subsequently relapsed was ungenerous, considering that chronic ulcers frequently relapsed whatever might be the treatment employed.

Dr. PEACHEY mentioned that Keetley, in his "Surgery," recommended Baynton's method of healing ulcers. Strapping was used to approximate the edges of the ulcers; it was not employed in order to procure rest, but to enable the patient to get about.

Section of the History of Medicine.

May 19, 1915.

Dr. NORMAN MOORE, President of the Section, in the Chair.

William Bromfield, 1713-1792.

By G. C. PEACHEY.

OUR knowledge of William Bromfield's career has hitherto been limited to the memoir in Rees's Cyclopædia published in 1819, which the compiler says "contains all the memorials which we have been able to collect of this ingenious gentleman, of whom no life has been before written." This article carries the appearance of having been contributed by one of Bromfield's contemporaries, but one, forsooth, who had but a slight acquaintance with his personal affairs, for there is scarcely one of his statements which will bear the test of investigation; and what is still more unfortunate, is that on turning to the "Dictionary of National Biography," we find that the author of the memoir therein has apparently commandeered Rees's statements without inquiry, and has thus added the stamp of his own authority to what possesses no more than the similitude of authenticity.

William Bromfield's personality has more than ordinary interest, for not only did he achieve distinction in his profession—he was Surgeon to Frederick, Prince of Wales, and afterwards to Queen Charlotte—but he has been credited with the right, had he chosen to exercise it, of assuming the baronetcy of Bromfield of Southwark, co. Surrey, which had been conferred upon that family at the Restoration. The fact stands out that he did not do so, and the mystery of his presumptive right—which recalls the doubtful baronetcy of Edward Greaves, M.D.—has exercised sufficient fascination to induce me to unravel the tangled skein of his history and family descent, rather than to adopt, as a servile copyist, the information which is already published.

His obituary notice in the *Gentleman's Magazine* runs as follows: "24 November, 1792, aged 80, in Conduit St., William Bromfield, Esq., by right a baronet, and what is of more consequence, by merit one of the most eminent surgeons of the present time"; while that in the *European Magazine* reads: "William Bromfield, Esq., in Conduit St., age near 80, formerly an eminent surgeon and author of several pieces relative to his profession, particularly on inoculation for the small-pox. He is said to have been by right a baronet, and was the alterer of an old play, 'The City Match,' acted one night at Drury Lane for the benefit of an hospital."

In Courthope's "Synopsis of the Extinct Baronetage," published in 1835, under "Bromfield of Southwark, co. Surrey," we may read: "The title on the decease of Sir Charles Bromfield, the third baronet, is said to have vested in the person of Philip Bromfield, a tea-man in Lombard Street, and on his death without issue to have descended to William Bromfield, Esq., surgeon to the Princess-dowager of Wales, but neither of these gentleman chose to assume it." This statement has been included in the "Complete Baronetage" of G.E.C. without comment or explanation; but, on the other hand, no mention of Bromfield's presumptive right is made either by Rees or the D.N.B. copyist, and although it appears from his obituary notices that he was popularly credited with that right, it is well-nigh certain that he did not assume the title, for only in the British Museum Index Catalogue has he been met with as Sir William Bromfield. This ascription of the prefix would claim greater respect were it not that the same cataloguer, while giving the benefit of the doubt to Sir Edward Greaves, Bt., has denied his authenticated title to Sir John Hill.[1]

[1] Edward Greaves, M.D., and Fellow R.C.P., is believed to have been created a baronet by Charles I, at Oxford, by patent dated May 4, 1645. In the Annals of the College; on the title-page of his Harveian Oration, which bears the imprimatur of the then President and Censors; in the list of Fellows appended to the Pharmacopœia of 1677; by Dr. Thomas Guidott, of Bath, a contemporary author; in the pedigree of the Greaves family inserted in Nash's "History of Worcestershire"; by Ward, in his "History of the Gresham Professors"; and in the list of benefactors to Bath Abbey, he has been styled a baronet. Furthermore, the actual patent is said to be in the possession of the descendants of one of his daughters, and, that being so, Le Neve, Norroy King of Arms, gave it as his opinion that the lack of enrolment was due to the rolls having been taken into the possession of the Parliament—as was the case with several other persons who received honours about that time. On the other hand, Anthony Wood calls Greaves "a pretended baronet," and his name is omitted from all the printed lists of the baronetage, except from the fifth edition of Guillim's Heraldry. Dr. Munk, however, in his "Roll of the College of Physicians," from which most of these particulars are taken, decided in favour of his baronetcy, and in such case Sir Edward Greaves was the first English physician to receive an hereditary title. John Hill, M.D., Aberdeen, and Fellow of the Academy of Sciences at Bordeaux—of which latter title he was

But Bromfield's alleged repudiation of a title and the advantages which it would have brought to him in his profession is not in any way consistent with the estimate of his character which I originally formed from a perusal of his printed works, of the published accounts of the professional disputes in which he was embroiled, and of the single letter written by him to Lord Bute, which constitute the few contemporary records at the biographer's disposal; and the result of the further search which I have made, besides elucidating various details, hitherto unpublished, of his family and descent, confirms my early suspicions, and demolishes once and for all the figment of William Bromfield's self-repudiated title.

In the first of the appended pedigrees is seen the descent of the family of Bromfield, of Southwark, co. Surrey, one of whom was created a Baronet at the Restoration. Upon the death of the second baronet without issue, the title passed to his nephew, the son of his deceased brother Charles, upon whose death, in 1733, the baronetcy became apparently extinct. *Any possible claimants must have been descended from one of the remaining brothers of the second baronet*, and it is only necessary to notice the dates at which each of these brothers was born: Sir Edward in 1632, Charles in 1643, George in 1647, and John in 1660. It is also requisite to add that a comparative study of the several wills and other documents places it beyond doubt that Sir John, the first baronet, had no other than these four sons.

The second pedigree begins with Thomas Bromfield, citizen and apothecary, of St. Andrew's, Holborn, who was twice married: firstly in 1667, according to his marriage licence at the age of 24; and secondly, in 1677, according to his marriage licence at the age of 34: the double entry proving beyond doubt that he was born in or about 1643; whereby it is certain that he could not have been the son of either of the four brothers mentioned above, the eldest of whom was not born until 1632, *only twelve years earlier*.

The paternity of Thomas Bromfield, the apothecary, has not been traced; all that is necessary for my purpose is to show, as the pedigrees do show, that he was not in direct descent from the first baronet, in

very proud—is known to medical history as physician, playwright, and vendor of nostrums. In return for a copy of his botanical works, superbly bound, the King of Sweden offered him the Knightly Order of the Polar Star, but his eccentricities coming to the ear of the Swedish Ambassador, he had to be content with the lower Order of Gustavus Vasa, leave to accept which decoration was procured from George III, and Hill was actually received and recognised at a levée as a Knight of that Order. Yet in the British Museum Index Catalogue his name appears as "Hill, John, M.D., calling himself Sir John Hill."

I.—Pedigree of Bromfield (Baronet) of Southwark, co. Surrey.

Fourth in descent from David Bromfield, who was himself fourth in descent from Griffith, ap David, ap Jerworth, ap Griffith, ap Cadwgan, "decka" from Ithell Velin of Yale and Liskell, 5th son of Llwelyn Aurdurchog of Bromfield and Yale.

Thomas Bromfield, of Odemere, co. Sussex. = Grace, da. and heir of Thomas Martin in co. Surrey.

1. John Bromfield, of Ewhurst, co. Sussex. =
2. Sir Edward Bromfield, knighted as Lord Mayor of London, 1637. Citz. and Fishmonger. Of Southwark. = Margaret, da. and coh. of John Paine, of Southwark, Gent.
3. Robert Bromfield. One of the examiners of the Star Chamber. =
4. Thomas Bromfield, ob. s.p.
5. Jerome Bromfield, ob. s.p.

Joyce, da. of William Austin, of Cheversall, co. Essex. = 1 *Sir John Bromfield*, 1st Bart., of Southwark, co. Surrey. Created March, 1661. Ob. *c.* July, 1666. Will P.C.C. 1666. Married secondly, Elizabeth, da. and heir of Wm. Michelborne, of Westmeston, co. Sussex, Gent. = 3 Anne, da. of Christopher Woodward, of Lambeth, arm. She married, in 1668, Anthony Collins, of Middle Temple, Esq. Mar. Lic. September 8, 1668, æt. 45. Ob. June, 1681. Will P.C.C. 1681.

Edward Bromfield, ob. s.p.

2

1. *Sir Edward Bromfield*, 2nd Bart., of Southwark, co. Surrey. Born *c.* 1632. Ob. s.p.m. February 17, 1703-4. = 1. Susan Bowell, of Ducklington, co. Oxford. 2. Anne, relict of William Lant, of London, merchant. Will P.C.C. 1696.

Elder s. by second marriage.

4. John Bromfield, of Isleworth and Inner Temple. Born *c.* 1660. Mar. Lic. July, 1682, æt. 22. Will P.C.C. 1683. = Eleonora Child, of Isleworth. She married (Mar. Lic. April 27, 1686, as "widow, of Isleworth") William Looker, of St. James's, Westminster.

Younger s. by second marriage.

2. Charles Bromfield, of St. George's, Southwark. Mar. Lic. January 31, 1667-8, æt. 25. Born *c.* 1643. Ob. *c.* 1674, v.p. Admon. P.C.C. 1674. = Mary, da. of Nicholas Salter, of London, merchant.

3. George Bromfield, of St. George's, Southwark. Mar. Lic. July 18, 1672, æt. 25. Born *c.* 1647. = Anne Williams, spinster, of St. George's, Southwark.

1. *Sir Charles Bromfield*, 3rd and last Bart., of Barton, co. Staffs. Born *c.* 1672. Ob. September, 1733, æt. 61. Will P.C.C. 1733. = Theodosia, da. and coh. of John Steele, of Orton, co. Leics. Will P.C.C. 1737.

1. Mary whose children the family = — Busby, inherited all estates.

[Extinct in male line.]

II.—Pedigree of William Bromfield, Surgeon (1713-1792).

Rebecca Girle, "of Merrybone." Mar. Lic. December 21, 1667. = (1) Thomas Bromfield, of St. Andrew's, Holborn, citizen and apothecary. Born 1643. Died 1711. Will P.C.C. 1711. = (2) Bridget, da. of Sir Thomas Witherly, M.D. Mar. Lic. February 16, 1677. Will P.C.C. 1707. (Had a cousin, Hannah Briggs, wife of William Briggs, M.D.)

1

Thomas Bromfield, senior, of St. Mary Woolnoth, druggist. Admon. P.C.C. 1721. = Margaret Scarth. Will P.C.C. 1750.

Philip Bromfield, of St. Mary Woolnoth, druggist and tea-man. Bur. Edmonton. Lands in Essex and Yorks. Will P.C.C. 1767. = Sarah. Died April 9, 1780. Bur. Edmonton. Will P.C.C. 1780.

William Heriot Bromfield, of London, M.D. Padua. Bapt. December 19, 1736. Bur. February 16, 1762.

1

Edward Bromfield, of London, Gent. Will P.C.C. 1712.

1. Mary = Cranmer.

2. Margaret.

Irene Heriot.

2

William Bromfield, of St. George the Martyr and of Gray's Inn, Barrister. Will P.C.C. 1744. = Mary Cranmer

(1) William Bromfield, of London, surgeon. Bapt. July 30, 1713. Bur. November 24, 1792, s.p.s. Will P.C.C. 1792. = (2) Anne Grover. Married 1758. (G. M. 1758, p. 45).

Charles Bromfield, of St. Mary's, Newington, surgeon. Bapt. April 10, 1748. Ob. vi. pat. Bur. St. Clement Danes. Will P.C.C. 1784. = Priscilla Gardiner, Bur. St. Clement Danes. Will P.C.C 1790.

3

Thomas Bromfield, junior, of London, M.D. Oxon. Will P.C.C. 1722. = Mary, da. of William Briggs, M.D. Admon. P.C.C. 1731.

James Bromfield, of St. Anne's, Westminster, apothecary. Died s.p. Admon. P.C.C. 1790. = Hannah ‖ Edmund Ferrers, and had issue. ↓

[Extinct in male line.]

which case neither he nor his descendants can have had any claim to the title. It may be that he was descended from one of the two uncles of the latter who are mentioned in the Visitation of London (1634), and appear in the pedigree, but even if that were the case, he would not have possessed any such claim.

Thomas Bromfield's eldest son, Thomas, became a druggist, married, and left issue one son, Philip, who succeeded to his father's business, and in 1733, the year in which Sir Charles, the third and last baronet, died, this Philip Bromfield was the senior living representative of the second family, and it was in him that Courthope tells us the title vested, though he did not choose to assume it. In his will dated 1764, in which he is described as "of St. Mary Woolnoth, druggist," he left to his wife Sarah, certain landed estates and also two messuages in Lombard Street, one of which was in his own occupation; while in 1776 we find his widow leaving a legacy of £100 to one "Robert Morshead, of Lombard St., tea-dealer, late a journeyman to my late husband." Philip Bromfield, tea-man then as well as druggist, of Lombard Street, and doubtless identical with the individual mentioned by Courthope, died in 1767 without issue, and on his death William Bromfield, the surgeon, his first cousin by the half-blood, succeeded to the representation of the family; though he, like Philip, did not assume the title for the best of all reasons—that neither of them had any feasible claim thereto.

How, then, to account for the origin of the fable? The last baronet had died in 1733, and Philip Bromfield, the tea-man, survived him for thirty-four years, yet so far as has been discovered no printed mention of any right to the baronetcy occurs until the death of William Bromfield, twenty-five years later, when it makes its first appearance in his obituary notices, but is disregarded by his earliest biographer, Abraham Rees, the cyclopædist.

Neither in Philip Bromfield's will nor in that of his widow is there any allusion to the title, while no suggestion of any such claim is to be found in William Bromfield's published works, or in his somewhat remarkable will; and, after a prolonged study of his character and career, the suspicion forces itself upon me that the story probably originated in the mind of William Bromfield himself, and that the choice of the name Charles, for his second son, a name unknown to three generations of his own family, was one phase, however passing, in the sequence of thought which ministered to his vanity by encouraging the popular idea to associate him with at least the right to a title.

Next to its assumption, which in default of proof his Court appointment rendered impossible, the popular belief in its self-repudiation for mysterious but unexplained reasons would, if I read his character aright, have brought to him, as it brings to certain types of persons, some peculiar gratification. To the very last, within six months of his death, William Bromfield preserved the tradition, for the print of his portrait by Orme, after Cosway, bears the arms of the family of Bromfield of Southwark, undifferenced—i.e., *on a shield azure*, and for crest alike, *a lion passant-gardant*—and it was this print of which in his will he ordered copies to be sent to his friends after his death, with the request that they should hang them up in their respective houses. He appears to have been the first of his family to bear arms, and though his son Charles copied his example, no evidence can be found in the Visitations that they were by right armigerous.

The four generations which are shown in the pedigree of William Bromfield's family demonstrate a remarkable predilection for medicine and the allied sciences. His grandfather, Thomas Bromfield, himself an apothecary, married the daughter of Sir Thomas Witherly, physician to Charles II, and President of the College of Physicians; and of his four sons, Thomas, the eldest, was a druggist, to which business, combined with that of a tea-man, his son Philip succeeded; while his youngest son Thomas, a Doctor of Medicine of Oxford, married the daughter of William Briggs, M.D., who taught anatomy to Isaac Newton, and was physician to William III and St. Thomas's Hospital. They left three children, William, the subject of this memoir; James who died in 1790, and was very probably identical with the James Bromfield who, about 1770, became one of the visiting apothecaries to the Lock Hospital;[1] and Hannah, to whose children William Bromfield bequeathed the reversion of his estate. Finally, each of William Bromfield's sons was a medical man, and we are thus confronted with the remarkable fact that of the ten males appearing in this second pedigree no fewer than eight were connected in one way or another with the healing art.

Doubtless by deducing the date of his birth from the account given in his obituary notices, Rees and the D.N.B. copyist have stated that he was born in London in 1712; but I find, from the parish registers, that he was born in New North Street, Holborn, and was baptised

[1] James Bromfield resided in Wardour Street, Soho, and dying intestate is described as "late of St. Anne's, Westminster," administration being granted to his principal creditor, Ambrose Godfrey, a well-known chemist and druggist of that period, who was one of William Bromfield's executors.

July 30, 1713. Nothing of his early career has been discovered, except that he was initiated into the practice of surgery by John Ranby, *to* whom he dedicated his first printed work on "The English Night-shades," and *from* whom he himself says he received the first rudiments of his chirurgical knowledge. He was not, however, apprenticed to Ranby (his name not appearing in the apprentice-books of the Barber-Surgeons' Company), which makes it probable that he was bound to a country surgeon.

It is said that he settled early in practice, and in 1736, being then barely 23 years old, we find him already married and residing in Fetherstone Buildings, which are still standing just opposite Great Turnstile in Holborn, and at the end of that year his eldest son, William Heriot Bromfield, was born. By this time he had commenced to lecture on anatomy, a specimen of his "Syllabus Anatomicus" containing twenty-two prælectiones, London, 4to, 1736, being included in the Library of the Royal Society of Medicine. Wadd says that he was lecturing on surgery as well as anatomy in 1739, while Rees and the D.N.B. copyist state that he began a course of lectures on those subjects in 1741; but nothing is certain except that, in 1743, a Syllabus Chirurgicus was added to the anatomical schedule of that year. In 1744 he was elected Demonstrator of Anatomy at Barber-Surgeons' Hall, and it was probably about this date that he bought the anatomical collections of Dr. Francis Sandys (formerly Professor of Anatomy at Cambridge), which he afterwards sold for £200 to William Hunter.[1]

His earliest connexion with St. George's Hospital was in November, 1742, when he subscribed five guineas, and in the following June he was elected a Governor. In June, 1744, he applied for one of the vacancies on the surgical staff produced by the resignations of John Wreden and of James Wilkie, whose name deserves to be rescued from oblivion as the first of William Hunter's surgical teachers;[2] and with William Hewitt, Bromfield was elected to that office on August 9, following, they having acted in the interim as "assisting surgeons" at the request of the Board. This appointment was probably obtained by Ranby's influence with the Prince of Wales, who was President of the Hospital, and whose protégé, Wreden, had then recently resigned, Bromfield succeeding him somewhat later as surgeon to the Prince and Princess. Little mention of his work appears in the Minutes of St. George's, except that it was he who suggested that copper warming-

[1] Simmons's "Life of William Hunter."

[2] *Idem.*

pans should be provided for warming the dressings, and that a stove should be placed in the operating theatre; and he was also answerable for some improvement upon the type of four-post wooden tester bedsteads then in use.

It is stated that in July, 1746, Bromfield summoned a committee of influential persons who resolved to form a society for establishing the Lock Hospital at the corner of what is now Chapel Street, Grosvenor Place, he being appointed its first surgeon, concurrently, be it noticed, with his occupancy of a similar post at St. George's. This office he continued to hold until his appointment to the Queen's Household in 1769 (at a salary of £150), about which date he became Consulting Surgeon to the Lock Hospital, his son Charles Bromfield succeeding him upon its active staff.[1] In this reference both Rees and the D.N.B. copyist state that in conjunction with the Rev. Martin Madan he formed the plan of this institution, though the latter's alleged co-operation in its foundation is apocryphal, for he did not leave Oxford until November, 1746, at the age of 21, after which, while reading for the Bar, he wasted some years as a member of a convivial London club, and it was not until he came under the spell of John Wesley's enthusiasm, and entirely reformed his morals and views of life, that he took orders (in 1750), eventually becoming Chaplain to the Lock Hospital. What is more probably correct is that, in conjunction with Madan, Bromfield was instrumental in procuring funds for the erection of the Hospital Chapel, which was built in 1761, and from which the present street takes its name; and it was for the benefit of the Lock Charity that Bromfield is credited with having altered—in collaboration with a friend, possibly Madan—an old comedy called "The City Match," by Jaspar Maine, which under the title of "The Schemers" is said to have been performed at Drury Lane in 1755.

In 1745 Bromfield was appointed, *vice* John Wreden, Surgeon to H.R.H. Frederick, Prince of Wales, the President of St. George's Hospital, after whose death, in 1751, he continued his services to the Dowager Princess; and in a volume of miscellanea among the Additional MSS. in the British Museum is a fragment of a letter which, though the signature is missing, is stated to have been written by William Bromfield to Lord Bute, then recently appointed Secretary of State.

[1] The writer regrets that he is unable to obtain further information concerning the association of the Bromfields with the Lock Hospital, since the Board of Management declined to allow him access to the hospital Minutes. On the other hand, he desires to express his obligations to the Rectors of St. George's, Hanover Square, and St. Andrew's, Holborn, for permitting him to search their parish registers free of charge.

Conduit Street, July the 14th, 1761.

My Lord,

Being greatly sensible how much your Lordship's time is engaged in the service of our King & Country I dare not presume to beg an audience, but as your lordship was some time since made acquainted with my Services in the late Prince of Wales's Family, & his royal Promise to provide for me in the way of my Profession it will be unnecessary to intrude longer on y^r^. . . . . (*four lines are here excised*) . . . . . her Surgeon; y^r^. Lordship's known Justice and Goodness to the Servants in general of my late Royal Master, has encouraged me to be thus troublesome to y^r^. Lordship & rely on y^r^. Lordship's great Benevolence for Pardon if, thro' ignorance, I have behaved improperly in this address to y^r^. Lordship, as I hope on every occasion that offers to convince y^r^. Lordship I am, My Lord, y^r^. Lordship's . . . . . [1]

The redundancies which characterise this letter—usual though they were at that period—afford some indication of Bromfield's regard for titles and titled persons; and there can be little doubt that he took the opportunity of Lord Bute's preferment to the post of Secretary of State in March, 1761, shortly after George III's accession, to approach him first through a friend at Court—probably John Ranby—and afterwards by letter, with a view to obtaining some Court appointment. It is probable that the words "her Surgeon," with which the latter portion of the letter commences, refer to the Princess of Mecklenburgh, afterwards Queen Charlotte, and that it was as the result of these applications that Bromfield was appointed one of the party which started for Strelitz, in the following August, to attend the Princess on her journey to England to marry George III. It was not, however, until after Gataker's death that Bromfield was appointed, in 1769, Surgeon to Her Majesty's Household.

In 1760 he was chosen one of the Court of Assistants of the Surgeons' Company, and in 1768 one of the Court of Examiners and Senior Warden, in which latter capacity, the Master being absent, he presided over the Court from whom John Hunter received his diploma to practise on July 7 of that year. Curiously enough, in no life of Hunter with which I am acquainted does any mention occur of his examination for this diploma, and it is worthy of remark that he had already been practising for nearly five years, and that it was probably only in view of his determination to apply for the next vacancy on the staff of St. George's, that he then submitted himself for examination. For Hunter was by this time 40 years of age.

[1] British Museum, Add. MS. 5726 C.

In 1769 Bromfield was chosen Master of the Company for the ensuing year, and in June, 1780, being then 67 years old, he sent in his resignation as a member of the Courts of Assistants and Examiners; and although he was requested to reconsider his decision, he failed to answer the letter or acknowledge the compliment it conveyed. Among the apprentices entered under him at Surgeons' Hall were William Heriot Bromfield (his elder son), 1753; Venner Hartcup (House Surgeon at St. George's in 1759), 1754; David Bayford (Assistant Surgeon at St. George's), 1759; John Bromfield Ferrers (his nephew, afterwards in Holy Orders), 1773; and John May, 1777. His earlier pupils included S. Howard, 1752; — Wright, afterwards of Sheffield, to whose surgical skill he refers in his "Observations," and Christopher Wren, who is elsewhere mentioned. But although the pupils' register at St. George's commences with the name of John Hunter, in 1756, it is impossible to differentiate the surgeons under whom each was entered until March, 1773, after which date the following were introduced to the Board by Bromfield as his pupils:—

1773	Thomas Robinson.	1775	John Kelly.	1777	Edward Welchman.
	Thomas Brand.		Thomas Griffiths.		Peter Hendley.
	George Iveson.		William Badley.		William Lunn.
	Francis Fortescue.		Thomas Grey.		John Haviland.
	John Scholey.		Robert Bluham.		John Lake.
	Thomas Palmer.		Nathan Dean (6).	1778	Christopher Batham.
	John Ryan.		James Cunningham.		Thomas Armstrong.
	Sylvester Jepson.		Joseph Pearse (6).		Thomas Robinson.
	Thomas Rogers.	1776	Nich. Tripe.		Richard Daston.
1774	D. Watkinson (6).		Thomas Seeds.	1779	Andrew Nixon.
	Philip Gresley.		Mr. Poppelwell.		Thomas Lewis.
	James Bowen.		Robert Hamilton (3).		James Berry.
	Roger Starkey.		George Grant (6).		William Gaggs.
	Gervas Wylde.		Henry Wastell (6).		George Hughes.
	Richard Bragg Blayden.	1777	Thomas Pripeck (3)	1780	George Hicks (6).
	John Barker (6).		Samuel Randall.		John Pearson (6).
	George Spence.		Francis Holmes.		Henry Braddon (6).
	Thomas Metcalf.		William Dance.		—
	Richard Newman Toll.		William Westby.		[Wm. Bromfield,
1775	John Smith.		John Jones.		resigned November 1, 1780].

Each was admitted for twelve months, except those otherwise marked.

In 1773 David Bayford was elected assistant surgeon to Bromfield by special resolution and ballot of the Board of Governors of St. George's Hospital; but on October 21, 1778, Bayford resigned and Bromfield's son Charles succeeded as assistant surgeon to his father until November 1, 1780, when William Bromfield sent in his resignation, and the thanks of the Board were ordered to be conveyed to him "for his long,

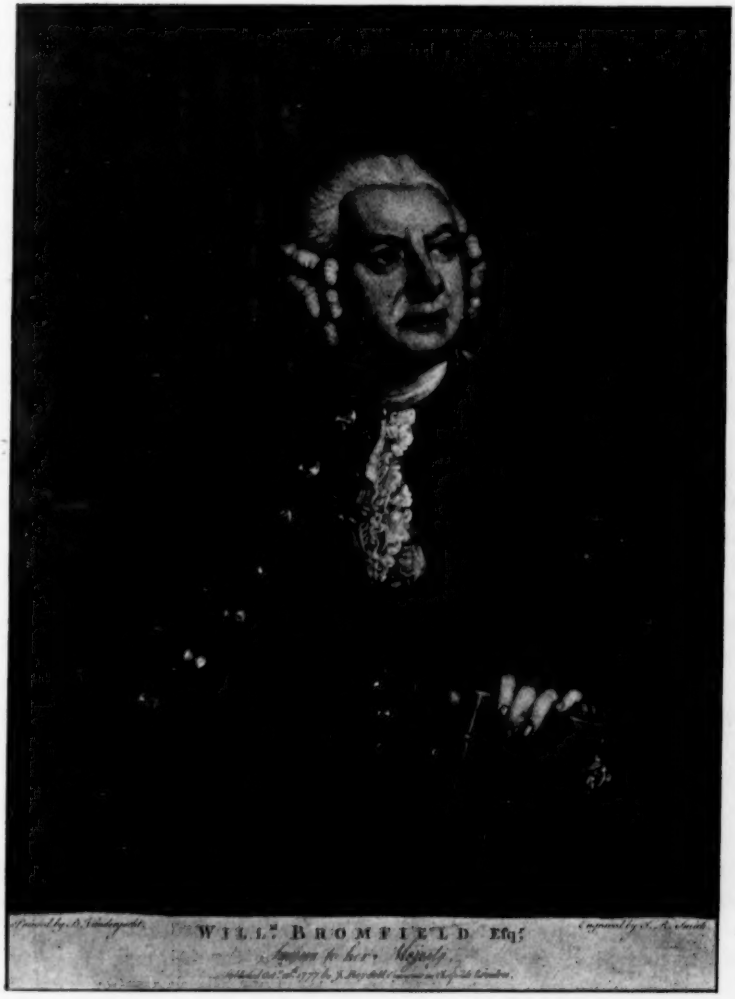
benevolent, and highly useful assistance to the Hospital as a surgeon and Governor, of which services the Governors will always retain the most grateful remembrance." He had served in that capacity for thirty-six years.

Ottley says: "The super-eminence of Percival Pott overshadowed the position of the rest of his professional brethren, but the second stations were ably filled by Bromfield and Sir Cæsar Hawkins of St. George's, and Samuel Sharp and Warner, of Guy's. These divided amongst them the greater part of the civil practice, whilst Adair and Tomkins, from their long connexion with the Army, enjoyed the chief share of what accrued from that quarter." [1]

In February, 1773, Bromfield was called in without avail to extract a bullet from the body of Lord Bellamont, who had been wounded in a duel by Lord Townshend; and in August, 1775, he was summoned as a witness for the defence in the trial of Miss Jane Butterfield, on a charge of murdering Mr. William Scawen by the administration of corrosive sublimate, upon the therapeutical properties of which agent Bromfield had written a treatise.

But in spite of the eminence he attained we have comparatively little knowledge of his professional career, and were it not for his published works we should have no more to record of him than we have of his more illustrious colleague, Sir Cæsar Hawkins, who so far as is known contributed nothing to literature. Bromfield dedicated his "Chirurgical Observations and Cases" to Dr. Thomas Lawrence and was permitted to present a copy in person to George III, when he had the honour of kissing His Majesty's hand and probably experienced the disappointment of not receiving knighthood. This copy, now in the King's Library in the British Museum, is bound in red morocco, extra gilt, each volume bearing on its front cover the royal monogram; and when I first saw it some of its leaves were still undivided by the reader's paper-knife. Rees says that "though containing much judicious practice and many valuable observations, the work did not answer the expectations of the public and was attacked by an anonymous writer, said to be M. Justamond, whose keen and ingenious strictures prevented the sale of the book which the author's fame would otherwise have secured." Not even Pope himself could more effectually have damned his victim with faint praise than did the critic when he wrote: "Many of his remarks on fractures and diseases

[1] Ottley, "Life of John Hunter," p. 25.


WILL.m BROMFIELD Esq;
Surgeon to her Majesty

of the urethra may be useful to young people, and may be read not only with safety, but with improvement." Poor Bromfield! and this was his *magnum opus*. Therein he claimed to have been the projector of warm sea-water baths for persons of a scorbutic habit, a term which embraced almost all the chronic forms of disease then known to medicine[1]: he claimed to have introduced the method of tying the larger vessels without including in the ligature the accompanying nerves[2]: he claimed to have been the first to recommend the pushing back of the scapula at the same time that the arm is extended, in reduction of the dislocated shoulder[3]: he claimed to have brought in the use of pasteboard splints, moulded when moistened with vinegar to the shape of the injured limb[4]: he claimed to have devised the use of a strip of leather fastened at intervals on the outside of splints, through which moveable straps might be passed to hold the splint in position[5]: he claimed to have been the first to advise the relaxation of muscles in the setting of fractures, a method which he says was not practised by anyone else except William Sharp, of St. Bartholomew's, who assured him that he had adopted that measure ever since Bromfield had recommended it in his lectures[6]: and finally he invented two lithotomical instruments, one a *bistourie caché*, to divide the neck of the bladder, and the other, a double gorgeret, for cutting through the prostate gland, for which latter instrument he claimed greater safety and other advantages over the much better known "cutting gorget," which is traditionally associated with the name of his rival, Cæsar Hawkins, and was then used by almost every surgeon in England who operated for lithotomy.[7] In short, William Bromfield seems to have had a partiality for making claims, some of which would appear to have been as difficult to substantiate as was his presumptive right to a title.

His literary productions were mostly contentious, and in almost every instance elicited the publication of hostile criticism or spirited rejoinder, his first essay giving an "Account of the English Nightshades," being in itself in direct contradiction to the views expressed in the monograph upon the same subject then recently written by Thomas Gataker, the translator of Le Dran's "Surgery."

[1] Bromfield, "Chirurgical Observations and Cases," i, p. 79.
[2] *Idem*, i, p. 163.
[3] *Idem*, i, p. 280.
[4] *Idem*, ii, p. 109.
[5] *Idem*, ii, p. 115.
[6] *Idem*, ii, p. 110.
[7] *Idem*, ii, p. 241.

In 1759 he was accused of unprofessional conduct by George Aylett, a surgeon of Windsor, by whom he had been called into consultation, and, after the fashion of the time, a pamphlet was issued by each contestant, giving his respective version of the circumstances, each of course flatly contradicting the so-called facts adduced by the other. Rees considers that in Bromfield's "Narrative of Facts" he clears himself from the imputation of having treated his antagonist improperly; in the light, however, of the actuality that Bromfield wrote an apology to Aylett, acknowledging that his conduct had been irregular and contrary to professional ethics, there is at least room for a contrary opinion, which is somewhat strengthened by the discovery that his actions in another case, ten years later, gave rise to an epistolary conflict in which he was held up to censure. His "Thoughts on Inoculation," which were dedicated to Queen Charlotte, not only elicited an answer entitled, "A Vindication against the Arguments of Mr. Bromfield," but drew from a reviewer in the *Gentleman's Magazine* (xxxvii, p. 413) the caustic apothegm: "Mr. Bromfield is certainly a good surgeon, but he appears by this tract to be a very bad writer; for there is scarce one period in it that is not rendered defective, obscure, and inconclusive by false concord and syntax." His last and most important work, "Chirurgical Observations and Cases," has already been analysed; suffice it to add that not only was Bromfield accused by his critic of arrogating to himself discoveries and theories demonstrated by others, but also of perpetrating negligences and improprieties of style, and offences against language, grammar and common-sense.[1]

In one particular, however, Bromfield has been censured without cause. In 1772, he gave it as his opinion, founded upon observations he had made, that where aneurysm is found, except where it has been produced by external violence, the arterial system in general is diseased; and that by amputating a limb and ligaturing the artery immediately above the aneurysm, as was then the custom, another dilatation of the vessel is likely to be produced in that or some other vessel that happens to be diseased. It seems that Pott was of somewhat similar opinion, for he wrote that the artery is generally diseased some way above the dilatation, especially the popliteal; and that the want of collateral branches of sufficient size to carry on the circulation is another powerful impediment to the operation. In his "Chirurgical Observations and

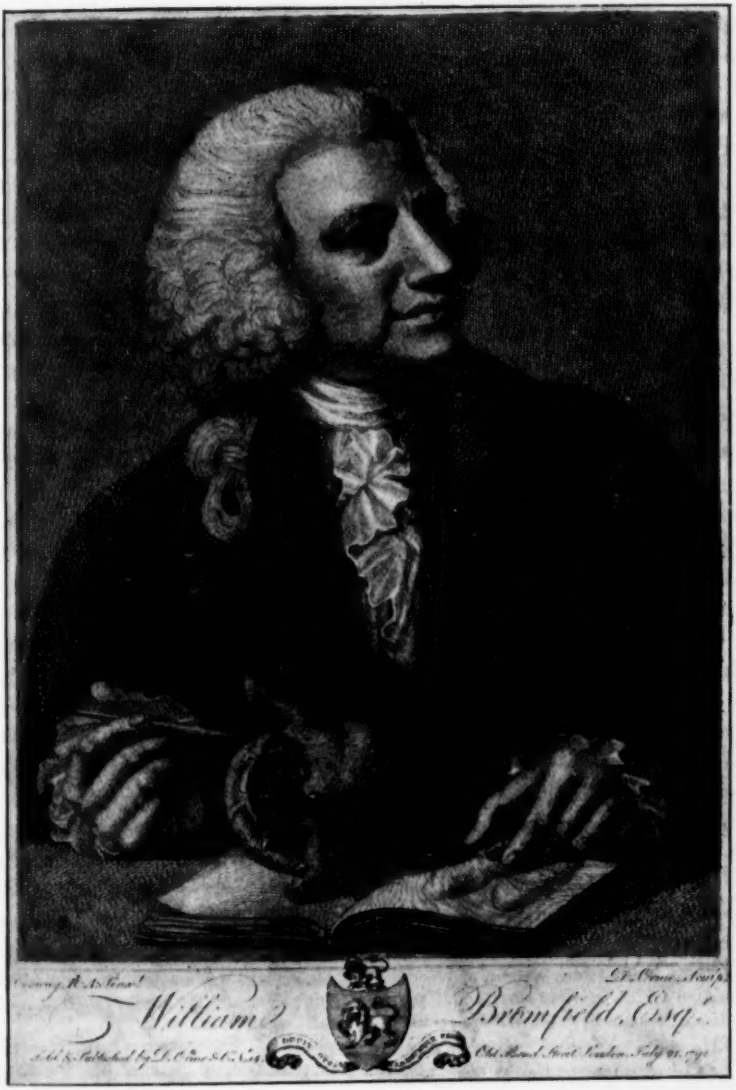
[1] D. A. S., "Notes on Mr. Bromfield's 'Chirurgical Observations and Cases,'" 1773, pp. 54, 58.

Cases,"[1] Bromfield, writing not later than 1772, says: "The most extravagant propositions have been suggested by some—namely, that the tying of the principal trunks of the arteries of any of the extremities, when wounded, may be done with a fair prospect of saving the limb. I once saw an attempt of this kind in a true aneurysm situated in the ham, on which I shall make no further remark than that the patient died, and I dare believe that the embarrassments which occurred, as well as accidents in the operation, will deter the operator from making a second attempt should a similar case offer for his assistance." The operator referred to here was none other than John Hunter, who had been appointed to the staff of St. George's, in 1768, as Bromfield's colleague. Now, in his lectures on surgery,[2] Hunter, when answering Bromfield's and Pott's objections, describes four cases of aneurysm, in the first of which the artery which he had tied burst above the ligature on the fifth day, and although he tied it again higher up, the man died two days later. And he proceeds to acknowledge that this was the case which led Bromfield to condemn the operation and to which he had referred, adding that he himself did not know why the operation failed, whether from the unsoundness of the vessel or otherwise. Yet by some oversight, Mr. Stephen Paget, in his life of John Hunter, stigmatises Bromfield as "unwise, because he advised that no operation should be done for aneurysm, and backed this advice by saying that in such cases the whole arterial system is diseased; and not straightforward, because the patient died fifteen months after the operation, neither of it nor of the aneurysm." But this latter patient, instead of being the one to which Bromfield was referring, was the first case in which Hunter performed the high operation in December, 1785, thirteen years after Bromfield's published criticism. The accusation, therefore, of want of candour, which is evidently due to a misapprehension, must not be placed to Bromfield's discredit; and erroneous though his views upon the pathology of aneurysm may have been, he at least erred in good company—that of Percivall Pott.

In a somewhat remarkable will, dated about six months before his death, Bromfield directs that his personal property, with the exception of his house in Conduit Street and the furniture therein, shall be sold; the proceeds to be invested to pay an annuity of £200 to his much-esteemed friend, Mrs. Caterina Ryley, of St. James's Place, to whom

[1] i, p. 304-06.

[2] Palmer, i, p. 551.


William
Bromfield, Esq.r

he also leaves some jewels, the picture of his deceased wife, Mrs. Anne Bromfield, and that of his son Charles. And he adds: "But lest the malevolence of the world may suggest any ill-natured censures on my good friend, Mrs. Caterina Ryley, for these small marks of my gratitude for her repeated kindnesses to me by visiting me daily, when in health or when ill, I join with all that know her in testifying that she is a woman of unspotted virtue, of true religious principles, and of the strongest integrity and justice in all her dealings with all the world." His own portrait in crayons he bequeaths to Sir James Wright, Bt.,[1] and he directs that Mr. Orme, the engraver, shall send a copy of his portrait from a painting by Richard Cosway, framed and glazed in the best manner, to Drs. Warren, Austin and Savage, who he hopes will do him the honour of hanging them up in their respective houses. To his friend, John Griffiths, of Conduit Street,[2] who succeeded him as Surgeon to Her Majesty's household, and who was already indebted to Bromfield in the sum of £1,000, he leaves all his surgical instruments and MSS., and all recipes for various complaints, with permission to publish them if he thinks fit. The residue of his estate, subject to the annuity and bequests, he leaves to his nephews[3] in succession and their children, in default to his cousin, John Hobart Briggs, one of the witnesses to his will being Richard Cosway, the artist, then living in Stratford Place.[4]

In 1736, he was living in Fetherstone Buildings, but about 1745 he removed to Conduit Street, and though he had retired from practice for some years, he was still occupying those premises at the time of his death. He had lived there nearly fifty years. Rees says that about 1773, when he was about 60, "he took a spacious mansion in Chelsea Park, which he enlarged, altered and elegantly furnished, whither he retired after doing his business, which henceforth began gradually to contract into a narrower circle." In 1819 this house was occupied by Sir Henry Wright Wilson, and ten years later was described by Faulkner[5] as "a capital mansion surrounded by extensive pleasure

[1] This was Sir James Wright, of Woodford, co. Essex, who was created a baronet in 1772, in reward for his services as His Majesty's Resident at Venice. It is probable that he had befriended Bromfield's son, William, during his residence at Padua.

[2] He was subsequently Surgeon to St. George's Hospital.

[3] These were the children of his only sister, Hannah, who had married Edmund Ferrers, Esq., of Piltdown, co. Sussex.

[4] P. C. C. 598 Fountain. Dated June 7, 1792. Proved December 17, 1792.

[5] Faulkner's "Chelsea."

grounds on the northern border of the Old Park" (Chelsea), which latter covered about 32 acres, and appears to have extended between King's Road on the south, Fulham Road on the north, Church Street on the east, and Park Walk on the west. This house, however, is not mentioned in Bromfield's will, and it is probable that he had vacated it before his death.

William Bromfield was twice married, and had several children by his first wife, Irene Heriot, none of whom survived him; and in 1758 he married secondly, Anne Grover, but by her he appears to have had no issue. His eldest son, William Heriot Bromfield, was born in Fetherstone Buildings, Holborn, and was baptised at St. Andrew's, Holborn, on December 19, 1736. He was entered at Eton in 1753, leaving two years later;[1] but his name appears as being apprenticed to his father in 1753, at Surgeons' Hall. He graduated M.D. at Padua, became a member of the Academies of Botany at Cortona and Florence, and of the Royal Academy of Sciences at Paris, and it was he who published a French translation of his father's work on "The English Nightshades" in 1761; and to him was dedicated, by the editor, a Latin edition of his father's "Observations on Lithotomy" (extracted from his lectures on Surgery), published at Florence in the same year. This dedication extols his rare learning in every branch of literature and science, and refers to his studies under the illustrious Florentine Professor, Antonio Cocchi, the friend of Isaac Newton and Richard Mead. This son, William Heriot Bromfield, died in London in 1762, apparently without issue.

William Bromfield's younger son Charles, by his first wife, Irene Heriot, was born in Conduit Street, and was baptised at St. George's, Hanover Square, on April 10, 1748. He was entered at Eton in 1759, paying an entrance fee of three guineas.[2] He does not appear to have been apprenticed to his father, but on October 21, 1778, having received his diploma to practise from the Court of Examiners of the Surgeons' Company on August 20 previous, he was appointed assistant-surgeon to him at St. George's Hospital, which office he vacated on his father's resignation in 1780. In 1770 he succeeded his father as surgeon in ordinary to the Lock Hospital, in the Annual Reports of which institution he is described as "late surgeon to the Princess-dowager of Wales," an appointment to which he may have succeeded on his father's retirement therefrom, in or about 1770. He remained upon the staff

[1] Austen Leigh, "List of Eton Collegers," 1905.

[2] Austen Leigh, "Eton under Barnard," 1904.

of the Lock Hospital until his death, in 1784, when he left a widow but apparently no issue; and on the death of his father, eight years later, this branch of the family apparently became extinct in the male line.

Besides the portrait of his deceased wife, by Maria Verelst, and that of his son Charles by Peters, both of which he bequeathed to Mrs. Ryley, at least three portraits of William Bromfield himself existed. The first was in crayons by Coates, and was left by will to Sir James Wright, of which no further particulars are forthcoming. The second painted by B. Vandergucht, and engraved in mezzotint by John Raphael Smith in 1777; and the third, painted by Cosway, and engraved in line and stipple by Orme, in 1792—both of which are reproduced here—were probably not in the testator's possession when he made his will, since they were not among his bequests, and their after-history and present whereabouts are alike unknown. In addition to these, two busts in mezzotint may be seen in the British Museum, each of which is attributed in the catalogue to William Bromfield. These portraits of a young man are much alike in face and dress. The first is lettered "Surgeon Bromfield, Published February 19, 1774"; and the second is lettered, "Mr. Bromfield Surgeon, Published April 9, 1774, by W. Humphrey," but it is impossible to conceive that the person depicted here could be identical with the subject of Vandergucht's picture, which was engraved and published only three years later: furthermore, if they had been the portraits of William Bromfield, he would have been styled Esq., instead of Mr.; and since his elder son was a physician and died in 1762, there can be little doubt that they represent his younger son, Charles, then aged 26, after his portrait by Peters. At the last moment I have been informed that this portrait now hangs in the Board Room of the Lock Hospital, to which institution it is said to have been presented by the Rev. H. W. Peters, its painter.

There can be no doubt that William Bromfield was an able surgeon, and little less that to his surgical skill were mainly due the eminence and the honours he obtained. His early knowledge came from Ranby, and it is highly probable that it was to Ranby's interest with George II, whose favourite surgeon he was, that Bromfield owed his appointment to the Prince of Wales. But he probably learned something more than surgery from John Ranby, whose traditional lack of manner and coarseness of speech and humour may well have produced in Bromfield some of the arrogance and self-assertiveness which

appear to have been his chief characteristics in his dealings with his professional brethren, with whom he was certainly not a favourite. That he was jealous of his rivals is proved by his omission of all mention of Cæsar Hawkins from his chapter on Lithotomy; that he was arrogant and overbearing, by his controversies with Aylett and others; and that he could withal descend to sycophancy, by his letter to Lord Bute. His vanity was probably increased by his appointment to the Prince of Wales, and became the greater as further success attended him; but, on the other hand, it is probable that his own failure to obtain his Sovereign's favour, which was bestowed upon his juniors, galled and irritated him. These, however excusable, are not the attributes of a man of strong character; and manually efficient though he was, I have been unable to discover in him the possession of either exceptional intellect or extraordinary intelligence. It would seem that he was inclined to spurn literature and reading; in this and many other respects resembling John Radcliffe. No books are mentioned in his will, and had he possessed a library he would almost certainly have used an armorial book-plate, but I have never come across a copy.

His countenance in Vandergucht's picture is heavy, hepatic and severe; as depicted by Cosway it is decidedly more pleasing, but that was Cosway's fashion; and it is not surprising that the latter should have been the portrait which was chosen by Bromfield for presentation to his friends, though the former was possibly the more speaking likeness.

The associates of his later years appear to have been John Griffiths, afterwards Surgeon to St. George's, Dr. Richard Warren, his colleague at that institution, Dr. William Austin, Physician to St. Bartholomew's, and Dr. Thomas Savage, a prominent obstetrician who lived near him in Conduit Street; and while he seems to have been on terms of some familiarity with Richard Cosway, who not only painted his portrait, but was one of the witnesses to his will, he alludes in one passage to his friend Mr. Cipriani, the artist, who drew three of the plates which illustrate his "Chirurgical Observations." This latter work he dedicated to Lawrence, illustrious for his piety, his learning and his skill, for eight years President of the College of Physicians, the biographer of Harvey and Frank Nicholls, and the physician and intimate friend of Samuel Johnson, who described him as one of the best men he had ever known. Of the friendship of such a man as Thomas Lawrence Bromfield was justly proud, but the very fact that not even *his* patronage and the approbation it implied were able to shield the work from obloquy and ridicule shows the extent of its author's unpopularity with his professional brethren.

Bromfield's health was probably bad, and his portrait by Vandergucht gives the impression that at 65 years of age he was a dyspeptic, who lived well and suffered from gout as the result of his indiscretions. In 1769 he had excused himself from attendance upon the plea of continued ill-health, and he mentions in his will the daily visits whether he was ill or well of his much-esteemed friend Mrs. Caterina Ryley, to whom he left some pictures, some jewels, and an annuity of £200.

After all, he was quite alone in the world; he had outlived his generation; Ranby and Lawrence had gone; his wife, his brother, and his two sons were dead, and his sister and her children were his only surviving relatives, to whom he left the residue and reversion of his property. He had himself experienced the ill-natured censures of the world, and his endeavours to shield the companion of his old age from the malevolent imputations which he feared might be cast upon her chastity do credit to his foresight and to his instincts. William Bromfield, vain, rude, self-assertive, and overbearing though he may have been, was at heart a gentleman.

LIST OF PUBLISHED WORKS.

(1) "Syllabus Anatomicus Generalem Humani Corporis Partium ideam comprehendens in usum Tyronum Rem Anatomicam necnon Chirurgicam prosequentium Præcipue Designatus" (containing 22 Prælectiones). London, 1736, 4to, pp. 25. (R.S.M.)

"Syllabus Anatomicus Generalem Humani Corporis Partium ideam comprehendens quarum singula Praelectionibus annuatim habendis dilucide exponenda. Adjicitur Syllabus Chirurgicus praecipuas Chirurgicae operationes complectens; quae, quo modo perficiantur; et fasciae singulis aptentur, cum summa accuratione Demonstrandum" (containing preface, 2 engraved plates, 32 anatomical prælectiones, and 6 chirurgical prælectiones). London, 1743, 4to, pp. 60. (R.S.M.)

(2) "The Schemers, or The City Match, as it is performed at the Theatre Royal in Drury Lane, by Jasper Maine. Printed for the benefit of a Public Charity, 1755." London, 8vo, pp. 87. (B.M.)

[Dedicated to the President, Vice-Presidents, Governors, and Benefactors to the Lock Hospital near Hyde Park Corner. Contains an Advertisement to the Public.]

There is a second copy of this play in the B.M., the title of which is "The Schemers, or The City Match, a Comedy, as it is Perform'd ——. London, 1s.," n.d., 8vo, pp. 87, and (in MS.) "by Wm. Bromfield," without dedication or Advertisement to the Public.

(3) "An account of the English Nightshades and their effects: Observations on the use of Corrosive Sublimate and Sarsaparilla, and Hints on the cure of Lues Venerea by the Secretion of Urine instead of by Salivation." London, 1757, 12mo, pp. 94, 2s. (R.S.M.)

[The dedication to John Ranby is addressed from Conduit Street, November 29, 1757.]

This work was translated into French by M. Bromfield le fils (i.e., William Bromfield, jun.), M.D. Padua, and Member of the Botanical Society of Florence. Paris, 1761, 12mo.

(4) "A Narrative of certain Facts relative to the conduct of Mr. Bromfield, a surgeon of London, towards Mr. Aylett, a surgeon and apothecary of Windsor, during their attendance upon Mr. Benwell, at Eton." London, 1759, 8vo, pp. 40, 1s. (B.M.)
This elicited "A genuine state of a case in surgery, being a full refutation of certain particular facts related by Mr. Bromfield," by George Aylett, surgeon at Windsor. London, 1759, 8vo, pp. 35 and 7, 6d. (B.M.)
A further account of this dispute is appended to "The Dissection," *q.v. infra.*

(5) "Observationes ad lithotomiam attinentes aliaque Chirurgiae Monumenta" (by William Bromfield, sen.). Florence, 1761, 4to. (B.M.)
[This work was printed and published by Andreas Bonduccius, of Florence, who dedicated it to William Bromfield, jun.—a long, laudatory dedication in Latin. The B.M. copy contains, as appendices, "Reflections," by Grima, on the lateral method of lithotomy, &c., practised by William Bromfield, sen.; these latter being in Italian.]

(6) "Thoughts arising from Experience concerning the present peculiar method of treating persons inoculated for the Small-pox, &c." London, 1767, 8vo, pp. iii and 88, 2s. 6d. (R.S.M.)
[Dedicated, by permission, to Queen Charlotte. In the Introduction he speaks of his work on Solanum as his last publication, thus ignoring his "Narrative of Facts" and the "Observationes" published at Florence.]
This elicited "A Vindication of the New Method of inoculating the Small-pox against the arguments of Mr. Bromfield (and others)," by Giles Watts, M.D. London, 1767, 8vo, pp. viii and 66, 2s. (R.S.M.)

(7) "Chirurgical Observations and Cases." London, 1773, 2 vols., 8vo, 14s. (B.M.)
[Dedication to Dr. Lawrence.]
This work elicited: (1) "Notes on Mr. William Bromfield's 2 volumes of Chirurgical Observations and Cases, with an appendix addressed to Dr. Lawrence, by D. A. S. . ., M.D., and Professor of Surgery (said to be M. Justamond)." London, 1773, 8vo, pp. 72, 1s. (R.S.M.) (2) "Serious Considerations on some remarkable passages in a work lately published by Mr. B—— and presented to His Majesty." London, 1773, 8vo, pp. 43. (R.C.S.)
Each of these is a sarcastic criticism, the former ill-natured, the latter humorous.

In addition the following is to be found in the *Philosophical Transactions*:—
1741 (p. 460), "Remarks on the case of a poor woman who had a fœtus in her abdomen for 9 years."

Besides these, which comprise Bromfield's published works, are several pamphlets published in 1769, touching the death of George Clark, in which matter Bromfield's conduct was impugned. The following have been consulted:—

(1) "The Blow, or an Inquiry into the causes of the late Mr. Clarke's death, &c. (by Dale Ingram, Surgeon)." London, 1769, 8vo. (R.S.M.)
(2) [A Letter to Mr. Dale Ingram (confuting his arguments). Anon. London, 1769, 8vo. (R.S.M.)
(3) "An Appeal to the Public touching the death of George Clark, by John Foot, Surgeon." London, 1769, 2nd edition, 8vo. (R.S.M.)
(4) "Mr. Bromfield's Defence in answer to Mr. Foot's appeal," *Gentleman's Magazine*, 1769, p. 226.
(5) "The Dissection, or an examination of Mr. Ingram's 'Blow,' with some remarks on Mr. Bromfield's vindication of himself, and a brief account of his behaviour towards Mr. Aylett, surgeon at Windsor." London, 1769, 8vo. (R.S.M.)

DISCUSSION.

The PRESIDENT congratulated Dr. Peachey on his most complete monograph. Such papers were useful in filling gaps in our knowledge. He was familiar with the portrait of Charles Bromfield at the Lock Hospital, but hitherto knew nothing about him. Bromfield himself was not an interesting person, but he had his share in the lives of greater men, and thus enabled us to complete the history of such as Hunter, Pott, and Ranby. In reference to Bromfield's supposed claim to a baronetcy, he asked Dr. Peachey if he could throw light on the disputed right of Edward Greaves to the hereditary title. His portrait at All Souls' College, Oxford, was described as that of "The Pretended Baronet."

[Dr. PEACHEY'S reply to the President's question respecting Edward Greaves is contained in the footnote on p. 104.]

Dr. LEONARD GUTHRIE asked whether it was true that Bromfield was the first to abandon the practice of ligaturing nerves with arteries. If so, he was deserving of credit. The custom must have led to many painful stumps, but it was of extreme antiquity and was based on the belief that the nerves were hollow in order to contain animal and vital spirits, therefore ligature of the nerves was necessary lest the spirits should escape.

Lionel Lockyer.

By HECTOR A. COLWELL, M.B.Lond.

IN the north transept of Southwark Cathedral, close to the Harvard Chapel, are the monument and grave of Lionel Lockyer, a seventeenth century quack. The monument is perhaps the most prominent object in the transept, and includes as its principal feature a large semi-recumbent figure of the doctor. The face wears an expression of unctuous self-satisfaction, quite in keeping with what we know of Lockyer himself. His very epitaph is made into an advertisement for his wonderful pills:—

"Here Lockyer lies interr'd, enough his name
Speakes one, hath few competitors in fame
A name so greate so gen'ral it may scorne
Inscriptions w^{ch} doe vulgar tombs adorne
A diminution 'tis to write in verse
His eulogies w^{ch} most men's mouths rehearse
His virtues and his PILLS are so well known
That envy can't confine them under stone
But they'll survive his dust and not expire
Till all things else at th' universall fire
This verse is lost, his PILLS embalm him safe
To future times without an epitaph."

Quacks are impudent enough in our own day, but it is doubtful whether any recent example has quite reached this level.

The inscription on the pediment states that Lockyer—

"Deceast Aprill ye 26th, Anno DO: 1672. Aged 72."

Of his antecedents but little is recorded, and he would be a quite uninteresting person if it were not for his monument in what in his

FIG. 1.

Lockyer's monument in Southwark Cathedral.

time was the Church of St. Mary Overie, but is now the Cathedral of St. Saviour, Southwark. In addition to this, however, there are in the British Museum some very pretty little tracts; some of these purport to be written by Lockyer himself, and of course eulogise the pills; others are from a rival pill-maker and are eulogistic neither of the pill nor of its compounder, and together they provide an amusing sidelight on the times wherein they appeared. It is to Lockyer's rival that we

must look for such information about his earlier career as can be gleaned. According to this worthy, Lockyer's normal occupations had been successively tailor and butcher. Such crumbs of chemical or medical lore as he possessed, he picked up from one Molton of Hogg Lane, and his first effort in pharmacology is reported to have been colouring a well-known medicine with cochineal and passing it off as his own. The medicine which was so treated was said to be "*mercurialis vitæ*," a "common and very churlish medicine," and which in spite of its name was simply a solution of some salt of antimony. Our informant proceeds to add that while at Molton's, Lockyer wasted both his time and his money, since he omitted to keep any written record of his work; and further that another quondam fellow-pupil of his did "testifie how he [i.e., Lockyer] wrought upon the Philosopher's Stone till he became *sine re & spe*, and from thence took a trip and stood up close halled for the bay of Medicine in which he hath cast out his Sheet Anchor of a licence which yet (as all licenses granted from the Bishops do) send him packing eight miles off from London before it gives him leave to practice."

That is all I have been able to find about Lockyer personally; it is admittedly from a hostile hand, but as will be seen directly from his own writings, he was as arrant and impudent a humbug as one is likely to meet with.

The pills were vaunted in a tract, of which a copy survives in the British Museum; it is adorned with a full-page portrait surmounted by his coat of arms, and surrounded by the legend "Lionel Lockier Physitian," while beneath the portrait is this doggerel:—

> "The true Effigies you here behold
> Of Him who for avoiding others ill
> Hath gain'd a Med'cine far exceeding Gold
> And known to all the world as Lockier's Pill."

It may indeed have been known to such of the world as were aware of its existence, as "Lockier's Pill," since at the end of his pamphlet we are treated to a text from the Bible to the effect that "Absolom because he had no son to succeed him, he erected a *Pillar* and called it *after his own name* (2 Sam. xviii, 18). And I have had *sons*, but *They are not*, and so I shall call the pill after my own name, Lockier's Pill."

The title to the advertisement is by no means so modest as this, since it runs: "An Advertisement concerning those most excellent Pills

called, '*Pillulæ Radijs Solis Extractæ*,' BEING an universall medicine especially in all chronical and difficult Distempers, as by the ensuing discourse will most clearly appear: Truly and only prepared by Lionel Lockier *Licenced PHYSITIAN*."

Needless to say the composition of the pill and the method of preparation are not divulged; but an artful piece of suggestive impudence is the statement that he has given his secret to three persons, "whom in regard of their greatness I dare not mention, considering what Solomon saith 'The King's wrath is as the roaring of an Lion, but his favour is as dew upon the grass.' Prov. xix, 12."

The price of the pills was four shillings a box, in boxes sealed with the maker's coat of arms, and only to be obtained from some forty dealers in town and country, and whose names are duly set forth at the end of the advertisement.[1]

The pills can be taken by anybody, no one is too young or too old. The dose seems to have been something of a matter of chance, since four are a good ordinary dose, and contain "some six grains." The directions to initiates in the treatment are as follow: "In the morning fasting, about four or five o'clock, take two or three of these pills and fast four hours from Meat or Drink, and so increase one pill every day until you come to five or six pills, and so take them three days, and rest one, and then to it again." These, undoubtedly, are explicit directions enough, but people are recorded who took twelve at once and yet had no harm. This alleged innocuous character of his nostrum is a little at variance with some of Lockyer's other accounts of its action, to be mentioned in due course. In the meanwhile it may be noted that the dose for a one-month-old baby is one-eighth of a pill, but this may be increased, if the child can stand it, until the disease is vanquished.

It is needless to say that the pill can cure everything; it may even be taken by those in perfect health (according to Lockyer, he did so himself) as a "preservative against all accidents as contagious aires, for which it stands Centinel in the body, and not permitting any enemy of nature to enter; if you increase the dose its action is more active and visible, and exercises its vertues upon the body, by expelling the disease."

It would appear to have been capable of exerting a silent and

[1] One of these was John Partridge, of North Perrot in Somersetshire; it would be interesting to know if he was any relation of another of that name, by trade a cobbler and by profession an astrological almanack maker, who some forty years later incurred the lash of Dean Swift. As a matter of fact, in his "Bickerstaff" pamphlet, Swift refers to the association between almanack makers and sellers of "pills and drinks for the venereal disease."

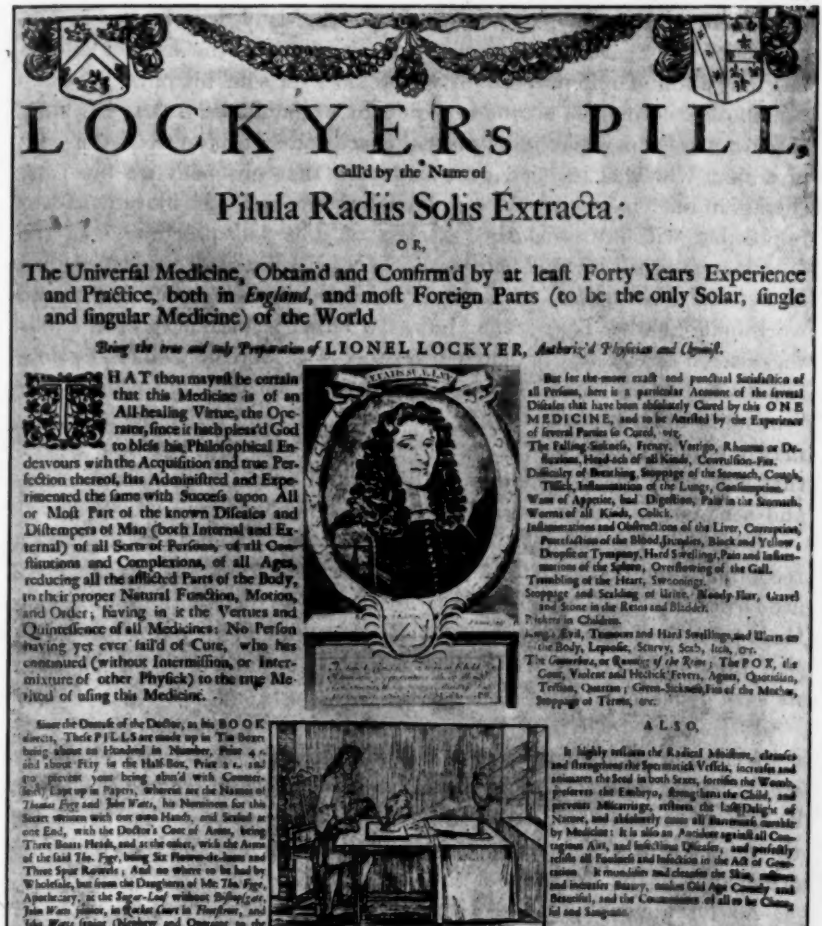
unknown, yet beneficent effect, but when the dose is sufficiently increased its effects are obvious enough, for "Sometimes it works upwards and sometimes downwards, and it may be both ways at once, although it is never violent." Most of us would think the dose had been increased rather too much when this pleasant state of things had been reached. Gout, leprosy, and dropsy will all receive benefit, but, with God's blessing, it is specially good for soldiers and sailors as a preventive against scurvy. What the military and naval surgeons of the time thought of the pills history does not record, but it does not require a very critical acumen to sum up, at its true value, Lockyer's statement that his invention will keep for a hundred years, and that a few boxes will save the lives of thousands!

Of course there are records of cures, and personal details of the patients are not wanting, though it must be admitted that they are more remarkable for accuracy of detail than for delicacy of statement. Thus: "Mrs. Dixon suffered for two years at least with a griping, gnawing pain in the belly, and by the use of my Pills, and God's blessing upon it, was cured; For before she had taken of my Pills six times she had a live worm come from her by Siege, four yards long; the woman lives in *Dead-man's Place* in *Southwark*, near unto the Colledge Gate. Her age is about thirty-two years, the worm came from her the latter end of May, 1662. If any desire to see the worm I have it by me."

Of course "thousands of cures" could be rehearsed, but the reader is advised that only such diseases as are curable by Nature and God's blessing are capable of successful Lockyerian treatment. The unfortunate incurables who lie under the ban of God, or Nature, or both, are those "whose bowels are so stuffed with ill humors that no *Medicine* will prevail: In some also, their Lungs are so rotten and putrefied, and the Liver also, before they think of a physitian or call for help." The Bible has again to be called in to prove the existence of incurable diseases, and we are forthwith treated to a long string of texts.

One deserving case seems to have specially enjoyed the Divine protection; it is such a triumph of impudence that I may be pardoned for a full quotation: "A young man told a friend of mine, that he had the POX,[1] who gave him two boxes of pills, and in three weeks time he was perfectly cured, although he scarce went to bed sober all that

[1] I am not responsible for either the syntax or the punctuation of Messrs. Lockyer and Starkey here or when elsewhere quoted.



LOCKYER's PILL,

Call'd by the Name of

Pilula Radiis Solis Extracta:

OR,

The Universal Medicine, Obtain'd and Confirm'd by at least Forty Years Experience and Practice, both in *England*, and most Foreign Parts (to be the only Solar, single and singular Medicine) of the World.

Being the true and only Preparation of LIONEL LOCKYER, *Authoriz'd Physician and Chymist.*

THAT thou mayst be certain that this Medicine is of an All-healing Virtue, the Operator, since it hath pleas'd God to bless his Philosophical Endeavours with the Acquisition and true Perfection thereof, has Administred and Experienced the same with Success upon All or Most Part of the known Diseases and Distempers of Man (both Internal and External) of all Sorts of Persons, of all Constitutions and Complexions, of all Ages, reducing all the afflicted Parts of the Body, to their proper Natural Function, Motion, and Order; having in it the Vertues and Quintessence of all Medicines: No Person having yet ever fail'd of Cure, who has continued (without Intermission, or Intermixture of other Physick) to the true Method of using this Medicine.

But for the more exact and punctual Satisfaction of all Persons, here is a particular Account of the several Diseases that have been absolutely Cured by this ONE MEDICINE, and to be Assisted by the Experience of several Parties so Cured, viz.

The Falling-Sickness, Frenzy, Vertigo, Rheums or Defluxions, Head-ach of all Kinds, Convulsion-Fits.

Difficulty of Breathing, Stoppage of the Stomach, Cough, Tissick, Inflammation of the Lungs, Consumption.

Want of Appetite, bad Digestion, Pain in the Stomach.

Worms of all Kinds, Colick.

Inflammations and Obstructions of the Liver, Corruption, Putrefaction of the Blood, Jaundice, Black and Yellow; Dropsie or Tympany, Hard Swellings, Pain and Inflammations of the Spleen, Overflowing of the Gall.

Trembling of the Heart, Swoonings.

Stoppage and Scalding of Urine, Bloody-Flux, Gravel and Stone in the Reins and Bladder.

Pissing in Children.

Kings Evil, Tumours and Hard Swellings, and Ulcers on the Body, Leprosie, Scurvy, Scab, Itch, &c.

The Gonorhœa, or Running of the Reins; The POX, the Gout, Violent and Hectick Fevers, Ague, Quotidian, Tertian, Quartan; Green-Sickness, Fits of the Mother, Stoppage of Terms, &c.

Since the Decease of the Doctor, as his BOOK directs, These PILLS are made up in Tin Boxes being about an Hundred in Number, Price 4 s. and about Fifty in the Half-Box, Price 2 s. 6d. to prevent your being abus'd with Counterfeit Lots up in Papers, wherein are the Names of *Thomas Fox* and *John Watts*, his Nominees for this Secret written with our own Hands, and Sealed at one End, with the Doctor's Coat of Arms, being Three Boars Heads, and at the other, with the Arms of the said *Tho. Fox*, being Six Flower-de-luces and Three Spur Rowels; And no where to be had by Wholesale, but from the Daughters of Mr. *Tho. Fox*, Apothecary, at the *Sugar-Loaf* without *Bishopsgate*, *John Watts* junior, in *Pocket Court* in *Fenchurch*, and *John Watts* senior (Nephew and Opposite to the Doctor at the time of his Death) in *S. Thomas's Southwark*. These are two Lions on the Pail at the Door, each holding a Shield, on which are painted Three Boars-Heads, and is the Dwelling House in which lived and died Dr. LOCKIER.

ALSO,

It highly refines the Radical Moisture, cleanses and strengthens the Spermatick Vessels, increases and animates the Seed in both Sexes, fortifies the Womb, preserves the Embryo, strengthens the Child, and prevents Miscarriage, restores the lost Delight of Nature, and absolutely cures all Barrenness curable by Medicine: It is also an Antidote against all Contagious Airs, and Infectious Diseases, and perfectly resists all Poysonous and Infection in the Act of Generation: It resolves and cleanses the Skin, softens and increases Beauty, makes Old Age Comely and Beautiful, and the Countenance of all to be Chearful and Sanguine.

At which Places, any Person may be furnish'd with the true PILL, with Allowance for Encouragement of the Sale thereof, in any Town or City of Great Britain or Ireland, where the same are not already sold.

They are also Sold by Retail, in several Cities in *England*, and particularly by Mr. *Horne*, Bookseller, and Mrs. *Gerraway*, Perfumer, both under the South-Gate of the Royal-Exchange, LONDON.

FIG. 2.

Old handbill advertising Lockyer's pills.

[*Photo. copyright.* Reproduced by kind permission of the Wellcome Historical Medical Museum.]

time, and within three weeks time he married a wife, and both of them very well to this day." We will hope the lady was happy in the society of her charming husband; but, at any rate, the extract affords fair indication of one purpose for which Lockyer sold his pills.

Another account of a confiding patient would indicate that his ideas of cure were somewhat indefinite in character. In brief, it is the case of a man who was in such continual pain that he spent all his time sitting in one chair with his legs upon another, and any movement was impossible without assistance. A box of pills was procured, and the dose increased till he took fourteen for a dose; even after this he seemed no better. Fortunately for Lockyer, he was a trusting soul and set to work upon another box. Now, however, either the strength of the pills was altered or else the patient's resistance was overcome, since the dose had to be reduced from fourteen to four at the outside. These four "wrought upon him so strongly both by *Vomit* and *Stool*, but then the man was in good hope of cure. Well he took a third box, and before that was *Half* spent, they broke him out all over his body into scabs, and sent out his distemper with a POX."

Lockyer's blasphemous impudence reached its limit in a passage recommending his stuff to the benevolent in town and country. They are advised to purchase a few boxes and then to distribute them among their poorer neighbours, whereby, "You shall manifest yourselves to be like unto our Saviour Christ, whom the Holy Scriptures record of, *That he went about doing good* (Acts x, 38)."

If Lockyer really had the episcopal licence to practise, their lordships' examination in midwifery was not exacting, and the method by which he describes his medicine as assisting difficult labour has every sign of originality, "for surely the *Medicine* being as yet in the Stomack, the mouth of the *Share* is opened and the folding doors of the *Os sacrum* are open'd in the *Loines*, and the *young* is presently expelled."

As an appendix to his advertisement, Lockyer inserts a letter purporting to be addressed from a "Person of Quality" to himself; notwithstanding its aristocratic origin, it commences "Honoured Doctor," and contains a vast amount of irrelevant matter both in English and in Latin. Among all this, however, is the statement that on June 13, 1664, Lockyer calcined the powder for his pill before the King and Court at Southampton House. There is nothing inherently improbable in the tale, since Charles, though interested in science, was by no means uninterested in various mountebanks and quacks—scientific and otherwise.

Lockyer, as will readily be conjectured, was not allowed to have matters all his own way. Another pill-maker, George Starkey, took up the cudgels on behalf of his own pill, and belaboured Lockyer unmercifully in a tract entitled "A smart scourge for a Silly Sawcy Fool." This Starkey was, perhaps, a higher class quack than Lockyer, and wrote M.D. (whence obtained I do not know) after his name. He had studied alchemy with the alchemist who wrote under the title of Eirenæus Philalethes; indeed, the two are sometimes confounded, but the truth appears to be that Starkey, after having purloined some of the papers and ideas of his master, published them as his own. He professed to have effected the transmutation of metals by means of a "medicine" given him by Philalethes, though he was unable to prepare it for himself. Starkey, having set up as a quack doctor, died in 1665 of the plague; Philalethes lived some years, and was intimate with the Hon. Robert Boyle.

Whatever may have been Starkey's professional attainments and qualifications, he is great in his contempt of Lockyer, his writings, and his pills, and it is in this tract that he refers to the last-named practitioner as a butcher and tailor turned physician. Lockyer's Latin and English were special objects of derision to Starkey, who plumed himself upon his attainments as a scholar and philosopher. The first part of the "Smart Scourge" is addressed to the "Person of Quality," who is the reputed author of the epistle in Lockyer's squib; and the opinion is pretty confidently expressed, that either that worthy himself, or some literary hack in his employ, is the author of both letter and pamphlet. It is perhaps unnecessary to state that his style is not remarkable either for refinement of expression or for solicitude regarding his opponent's feelings. "I find a letter of yours (like the close of syrup after a F——) concluding that tractate, usher'd in with the Licensed Physitian's encomium, that you are a Person of *Quality*, which although it might have been verified (had you been indeed but the common Hangman) yet my Charity bids me an higher esteem of you; that you are the true Father of the whole Scribble in praise of that silly cheat . . . . And truly Sir, professing myself a Scholar (which honor the worst of mine enemies dare not deny me), I will take notice first of your false Latine (a very bad *Quality*) for which, should I take you to task as a rigid Pædagogue, and make you untruss for the first fault, your Breech would be bloudy and too sore to sit on, if for all the lapses committed in that very short epistle you had (as you deserve) a several lash."

In another publication of Starkey's he is unkind enough to say that in the earlier copies of Lockyer's tract and advertisements the pills were referred to as "*Pilluradii extractus solus.*" Whether the subsequent emendation is due to the gentle pedagogical treatment prescribed by the Scholar is not mentioned.

Starkey published a tract upon his own remedy, entitled "George Starkey's Pill vindicated from the unlearned alchymist and other pretenders." Here again various fellow-quacks come in for a fairly vigorous abuse, notably one, Richard Matthews, who, sad to relate, is again "no scholar." Apparently in an unusual moment of good nature Starkey gave him the secret of his "Diaphoretic Diuretick Anodynous Elixir"; this, however, was between 1651 and 1655, and subsequently such improvements have been effected that poor Matthews and his preparations are quite out of the running. Starkey declares that his own pill is an opiate, in which there is a "true correction of the lethal qualities of opium." It is good for "Autumnal Agues, old Coughs, old Pains, old Gonorrhœas, and old inveterate venereall maladies, old obstinate uterine obstructions and the like." The sphere of its action, then, is clearly not circumscribed by one's usual conception of the action and uses of opium.

Matthews comes in for a few more compliments, and is informed that he himself knows how bad his pill is, and how carelessly made, "by which neglect, his Pills bind the body obstinately, and cause the next day giddiness, great drought, fainting, dejected appetite, suspension of wind, Sottish drowsiness, etc., for want of the closing hand of a true Artist."

DISCUSSION.

Dr. NORMAN MOORE mentioned that Sir William Church in an interesting paper in *St. Bartholomew's Hospital Reports* had stated that obsolete drugs such as "mummy" were to be obtained in London even at the present day.

Dr. F. PARKES WEBER asked whether anything was known about the ingredients of Lockyer's pills. An interesting communication would be one containing an account of what was known regarding the ingredients of ancient quack remedies.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE EIGHTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1914-15

LARYNGOLOGICAL SECTION

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1915

Laryngological Section.

OFFICERS FOR THE SESSION 1914-15.

President—

G. William Hill, M.D.

Vice-Presidents—

W. Jobson Horne, M.D.
H. Lambert Lack, M.D.
Sir William Milligan, M.D.
D. R. Paterson, M.D.
E. B. Waggett, M.B.
Sir Robert H. Woods, F.R.C.S.I.

Hon. Secretaries—

Cecil I. Graham, F.R.C.S.
W. D. Harmer, M.C.

Other Members of Council—

James Donelan, M.B.
Thomas Guthrie, F.R.C.S.
C. W. M. Hope, F.R.C.S.
W. H. Kelson, M.D.
E. A. Peters, M.D.
F. A. Rose, F.R.C.S.
Sir StClair Thomson, M.D.
George Wilkinson, F.R.C.S.
Andrew Wylie, M.D.

Representative on Library Committee—

Dan McKenzie, M.D.

Representative on Editorial Committee—

L. H. Pegler, M.D.

LARYNGOLOGICAL SECTION.

CONTENTS.

November 6, 1914.

	PAGE
By G. William Hill, M.D.	
(1) Fracture-dislocation of Right Great Cornu of Hyoid Bone	1
(2) Skiagrams of Frontal Sinuses operated on by Good's Method ...	2
(3) Skiagram of Frontal Region, showing Symmetrical Fronto-ethmoidal Cells, extending above Roof of Orbit	2
Diagrams (Lantern Slides) of Patients in whom the Inter-frontal Sinus Septum has been deliberately broken down by the Nasal Route so as to ensure more Complete Lavage for Double Frontal Sinus Suppuration. Shown by P. Watson-Williams, M.D.	3
Laryngeal Disease of Obscure Nature. By W. Jobson Horne, M.D.	5
By J. L. Irwin Moore, M.B.	
(1) Abnormal Overgrowth of Nose (Bulbous) in a Child, aged 2	6
(2) Male, aged 59, with Epithelioma of the Œsophagus, shown to illustrate the Benefits of Hill's Feeding Tube	8
Perithelioma of the Pharynx. By J. Coubro Potter, M.D.	9
Brain Abscess secondary to Frontal Sinus Suppuration; Drainage; Recovery. By Dan McKenzie, M.D.	10
Aspergillosis of the Maxillary Antrum (with Histological Report by S. G. Shattock, F.R.C.S.). By Herbert Tilley, F.R.C.S.	14
Intrinsic Epithelioma of the Larynx, suitable for Laryngo-fissure. By Sir StClair Thomson, M.D.	23
Cases of Pulsating Elongated Swelling on Lateral Wall of Pharynx. By W. Stuart-Low, F.R.C.S.	25
Hæmostatic Guillotine. Shown by G. J. F. Elphick	26
By H. Lambert Lack, M.D.	
(1) Deficient Tension of Vocal Cords	28
(2) Extensive Papilloma of the Trachea	29

By Hunter Tod, F.R.C.S PAGE

(1) Epithelioma of Right Tonsil 29

(2) Carcinoma of Palate 30

Ulceration of Soft Palate and Tonsil. By Walter Howarth, F.R.C.S. ... 31

Meningitis due to Absorption of the Roof of the Ethmoidal Cells on the Right Side corresponding to the Posterior Half of the Crista Galli. By P. Watson-Williams, M.D. 31

December 4, 1914.

Intrinsic Epithelioma of the Larynx One Month after Laryngo-fissure. By Sir StClair Thomson, M.D. 33

By Sir William Milligan, M.D.

(1) Demonstration of Foreign Bodies removed from Air Passages ... 38

(2) Inoperable Angio-fibroma; Maxillary Antral Sarcoma (?). (Microscopic Section) 42

A Naso-antral or Choanal Polypus, upon the Posterior Surface of which is an Ulcer the Size of a Shilling. By Herbert Tilley, F.R.C.S. 44

Retropharyngeal Swelling. By G. J. F. Elphick 45

By C. W. M. Hope, F.R.C.S.

(1) Tuberculoma of Nasal Septum 46

(2) An Unusual Case of Enlarged Tonsils 48

Two Cases of Injury to the Nose with subsequent Deformity. By W. Stuart-Low, F.R.C.S. 48

Tubes of Soft Metal for Insertion into Nose after Submucous Resection. By Dan McKenzie, M.D. 49

New Growth of Tonsil and Faucial Pillars, with Microscopic Specimen. By H. Clayton Fox, F.R.C.S.I. 50

February 5, 1915.

By Sir William Milligan, M.D.

(1) A Note on the After-treatment of Cases of Submucous Resection of the Nasal Septum without the Use of Splints 51

(2) Photographs of a Case of Chronic Lymphangitis of the Tissues covering the Nose; Hints as to Treatment desired 53

By E. D. Davis, F.R.C.S.

(1) A Nasopharyngeal Fibroma enucleated by a Curved Dissector and the Finger 55

(2) A Post-mortem Specimen of a Pituitary Cyst opened by the Killian-Hirsch Operation 57

(3) Report of a Case of Sarcoma of the Pituitary Body treated by the Killian-Hirsch Operation 57

(4) Post-mortem Specimens of a Malignant Growth of the Pituitary Body 59

PAGE

Laryngeal Obstruction following Specific Infection in a Young Man. By W. H. Kelson, M.D. 60

Fixation of Right Crico-arytænoid Joint; (?) Traumatic. By W. M. Mollison, M.C. 62

Report upon Sir StClair Thomson's Specimen of Laryngeal Growth, from case shown December, 1914. By S. G. Shattock, F.R.C.S. 63

Pachydermia Laryngis. By W. Jobson Horne, M.D. 64

By W. Stuart-Low, F.R.C.S.

(1) Dyspnœa, Perichondritis, Tumour (?) of the Mediastinum 64

(2) Epithelioma of Pharynx and Tongue 66

Epithelioma of Left Tonsil and Adjacent Part of Tongue after Operation (Specimen shown). By C. I. Graham, F.R.C.S. 67

Epithelioma of the Tonsil. By W. Jobson Horne, M.D. 68

Bullet Wound of Pharynx, &c. By C. W. M. Hope, F.R.C.S. 69

Specimen and Skiagram of a Case of Œsophagotomy. By H. L. Whale, F.R.C.S. 70

By H. Buckland Jones, M.B.

(1) Gunshot Wound of the Neck; Injury to Larynx, resulting in Formation of Web 71

(2) Gunshot Wound of the Neck, with Extensive Injury to Larynx ... 72

Shrapnel Wound of the Neck; Injury to Buccal Cavity; Exit beneath Angle of Jaw on Opposite Side. By W. Jobson Horne, M.D. 73

Synechiæ in the Nose treated by Diathermy. By Dan McKenzie, M.D. ... 74

Three Children illustrating Acute Suppuration of the Nasal Accessory Sinuses. By Herbert Tilley, F.R.C.S. 75

Tuberculosis of Nasal Fossæ. By C. I. Graham, F.R.C.S. 77

Tuberculous Ulcer of the Mouth. By H. Buckland Jones, M.B. 78

March 5, 1915.

Angeioma (Bleeding Polypus) of Nasal Septum. By William Hill, M.D. ... 79

By L. H. Pegler, M.D.

(1) X-ray Photographs illustrating the Thoracic Appearances in a Case of Left Recurrent Paralysis associated with Mitral Stenosis ... 80

(2) Specimens of Superior Maxillæ, exhibiting Abnormal Conditions of the Nasal Fossæ and Maxillary Antra 82

(3) Synechiæ and Contraction of the Vestibules 84

Tuberculous Inferior Turbinate. By Cecil Graham, F.R.C.S. 85

Bullet Wound through Face; both Antra and Septum perforated; Operation. By G. Seccombe Hett, F.R.C.S. 86

PAGE

By Sir WILLIAM MILLIGAN, M.D.

(1) Bone Impacted in Left Bronchus for Six Months; Removal; Recovery (Bone shown) 88

(2) Subglottic (? Tracheal) Growth; Removal; Recovery 88

(3) Dentigerous Cyst of the Lower Jaw; Operation 90

By H. L. WHALE, F.R.C.S.

(1) Perithelioma of Maxillary Antrum 91

(2) Patient, Gunner S., from whom the Piece of Tin shown at the last Meeting was removed by Posterior Œsophagostomy 93

Two Cases of Œsophageal Stricture in Children. By C. W. M. HOPE, F.R.C.S.... 93

Œsophagus from a Child, aged 2½, showing Escharotic Stenosis. By DAN MCKENZIE, M.D. 95

Epithelioma of Pharynx, Tongue, and Cervical Glands in Male. By W. STUART-LOW, F.R.C.S. 96

Rabbit's Vertebra in the Right Lower Bronchus removed by Superior Bronchoscopy. Shown by J. DUNDAS GRANT, M.D. 96

Removal of Epithelioma of Larynx. By W. M. MOLLISON, M.C. 97

Fibroma in Nasopharynx. By J. L. IRWIN MOORE, M.B. 98

Case for Diagnosis. By CYRIL HORSFORD, F.R.C.S. 100

May 7, 1915.

PART I.—CIVIL CASES.

Malignant Tumour at Base of Tongue almost dispersed by Radium. By WILLIAM HILL, M.D. 101

Case for Diagnosis. By W. M. MOLLISON, M.C. 101

Bent Nose Straightened by Submucous Rotation of the Cartilaginous Septum. By J. DUNDAS GRANT, M.D. 102

By IRWIN MOORE, M.B.

(1) A Nasopharyngeal Fibroma removed through the Mouth without any Preliminary Operation 103

(2) Growth in Maxillary Sinus extending into Nasopharynx 104

(3) Further Notes on a Case of a Gentleman, aged 59, with Malignant Stricture of the Œsophagus 105

Patient suffering from Recurrent Columnar-celled Carcinoma of the Antrum. By W. STUART-LOW, F.R.C.S. 106

New Growth in Post-nasal Space. By FREDERICK SPICER, M.D. 107

Halfpenny embedded in Œsophagus. By W. H. JEWELL, M.D. 108

Pharyngeal Fistula in an Old Man after Operation on Abscess in the Neck. By W. DOUGLAS HARMER, M.C. 108

PART II.—WARFARE INJURIES AND NEUROSES.

Introductory Paper by Major Sir WILLIAM MILLIGAN, M.D., and Major F. H. WESTMACOTT, F.R.C.S. 109

Exhibit—German Cartridge with Bullet reversed. By WILLIAM HILL, M.D. ... 115

GROUP I.—FUNCTIONAL CASES.

Two Cases of Functional Aphonia (one including Functional Deafness) following the Bursting of a Shell in Close Proximity to the Patient. By HERBERT TILLEY, F.R.C.S. 115

Gunshot Wound of the Neck with Laryngeal Symptoms for Diagnosis and Opinions as to Treatment. By COUBRO POTTER, M.D. 116

Functional Aphonia. By J. F. O'MALLEY, F.R.C.S. 116

Functional Aphonia. By H. L. WHALE, F.R.C.S. 117

GROUP II.—INJURIES OF NECK INVOLVING LARYNX, TRACHEA, ETC.

Laryngeal Stenosis following a Bayonet Wound treated by Intubation. By E. D. DAVIS, F.R.C.S. 120

By J. F. O'MALLEY, F.R.C.S.

(1) Two Cases of Paralysis of the Left Vocal Cord 121

(2) Traumatic Fixation of both Vocal Cords 122

Notes on the Case of a Soldier injured by a Horse's Kick. By W. STUART-LOW, F.R.C.S. 123

By WILLIAM HILL, M.D.

(1) Shell Wound of Neck 123

(2) Tracheal Obstruction due to long latent Cervical Abscess following Wound by Shrapnel Fragment 124

Case of an Officer shot through the Neck. By Sir STCLAIR THOMSON, M.D. ... 125

GROUP III.—INJURIES TO VERTEX, FACE, JAWS, NASAL CAVITIES, ETC.

By J. F. O'MALLEY, F.R.C.S.

(1) Bullet Wound in the Head 128

(2) Two Cases of Intranasal Adhesions 129

Bullet in Pterygoid Region of Skull. By WALTER HOWARTH, F.R.C.S. ... 130

Case of an Officer shot through the Cartilaginous Part of the Nose. By Sir STCLAIR THOMSON, M.D. 131

Injury principally to Lower Face and Mandible. By H. L. WHALE, F.R.C.S. ... 131

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

Laryngological Section.

November 6, 1914.

Dr. G. William Hill, President of the Section, in the Chair.

Dr. D. R. Paterson, in retiring from the Presidential Chair, said he desired to thank the members of the Section for the successful year he had had, and introduced his successor, Dr. G. William Hill, whose contributions to laryngology had materially enriched the *Proceedings*.

The President (Dr. G. William Hill), after expressing his thanks to Dr. Paterson for what he had said, and to the Section for his election to the Chair, referred to the lighting equipment for the examination of patients, and said it would not now be necessary for members to send specula and throat mirrors with the patients; enough had now been provided by the Society, and he hoped there would also be some to spare. If any member had any suggestions to make, or thought there should be more instruments, he invited communications, when every effort would be made to carry out such suggestions.

Owing to the war, the programme of the Section had been somewhat curtailed; there would be five meetings instead of the usual eight. He thought that at a time of stress like the present, when there would probably be a plethora of useful material, some subjects should be cut out which had already been thoroughly discussed, such, for instance, as diseases of the tonsils and adenoids.

Fracture-dislocation of Right Great Cornu of Hyoid Bone.

By G. William Hill, M.D.

This man fell off a bicycle one year ago and contused his neck. Pain for some days on left side. The right great cornu is now seen projecting into the right glosso-epiglottic fossa. The man has suffered from laryngitis with hoarseness ever since the accident.

Skiagrams of Frontal Sinuses operated on by Good's Method.

Shown by G. William Hill, M.D.

Dr. Watson-Williams's metal sounds were seen in situ. Good's bone files or rasps for enlarging the frontal ostium were exhibited.

Dr. Hill had recently performed Good's type of per-nasal operation for enlarging the frontal ostium on five frontal sinuses, using Good's special bone rasps which he had just exhibited. He was bound to admit that the technique appeared to be both easy and safe. It was too early to speak of the permanent results, but the immediate relief was surprisingly good. No doubt in some cases a larger and more direct opening could be obtained by Dr. Watson-Williams's drill and burr, and by other modifications of Ingals's original instruments, which were exhibited many years ago by Dr. Dundas Grant.

Skiagram of Frontal Region, showing Symmetrical Fronto-ethmoidal Cells extending above Roof of Orbit.

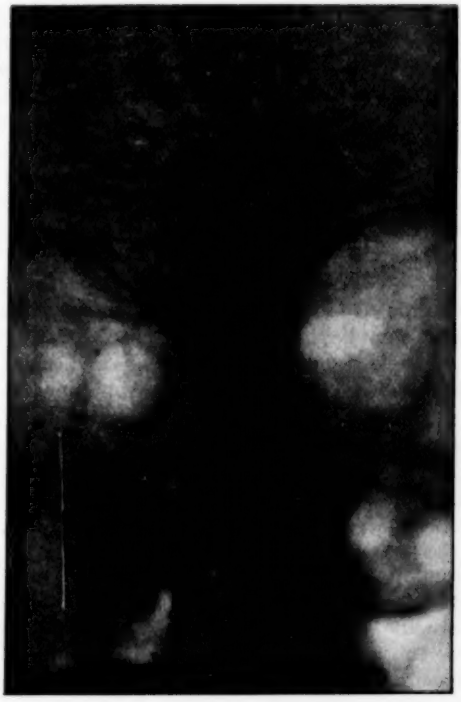
Shown by G. William Hill, M.D.

In reference to this skiagram, showing accessory fronto-ethmoidal cells extending far outwards between the frontal sinus floor and roof of the orbit, it had been assumed hitherto that the occasional presence of these cells, first described by Sir StClair Thomson under the name "orbito-ethmoidal," could only be ascertained by the Killian technique, and this assumption had been used as an argument for the inadequacy of the Ogston type of operation. The cells, however, could be seen by radiography quite clearly, and could be at least drained by Good's type of operation.

Diagrams (Lantern Slides) of Patients in whom the Inter-frontal Sinus Septum has been deliberately broken down by the Nasal Route so as to ensure more Complete Lavage for Double Frontal Sinus Suppuration.

Shown by P. WATSON-WILLIAMS, M.D.

FOUR skiagrams were shown. Dr. Watson-Williams pointed out that the rasp or sound shown passing up one frontal-nasal passage and through the intersinus septum to the opposite side appeared to be



Skiagram showing the author's small frontal sinus rasp passed into the (patient's) left frontal sinus, and, through the large hole it has made in the sinus septum, well into the opposite frontal sinus (the skiagram reverses the right and left sides). From a skiagram by Captain James M. Taylor, R.A.M.C., Skiagraphist, Bristol Royal Infirmary, 2nd General Hospital.

traversing the sinus septum lower down than was actually the case, this illusion being due to the fact that the head was tilted back, hence everything in a plane posterior to the anterior surface of the sinus of course would appear lower in a skiagram than the corresponding part of the anterior sinus wall.

DISCUSSION.

Dr. DUNDAS GRANT said it was pleasing to him to see the growing tendency towards the treatment of the frontal sinus by the intranasal method. When years ago he uttered an opinion in favour of it, there was a smile on the faces of the listeners. He might claim some priority in regard to the method just described, because at their meeting in Sheffield he brought forward an identical frontal sinus bougie, and narrated some cases in which he had found those bougies of considerable benefit. The only change necessary was to have the instruments made more pliable. The rasp was a great addition, and he recently used it in a case with much satisfaction, and he approached closer to guaranteeing a natural sized opening than he had done before. Rasps and burrs, however, were apt to provoke considerable granulation, compared with chisels and other clean-cutting instruments. Mr. Tilley had stated how useful he found strong solutions of nitrate of silver; strong solutions of argyrol were also very good. Also, while it was fashionable, one should use scarlet-red ointment. In a recent frontal sinus case he thought it materially assisted epithelialisation on the surface of the opening. In some of the cases on which he had operated radically he found that even then the passage contracted, and he had been glad to resort to his frontal sinus bougies. In a recent case he used them for what he thought to be mucocele. There was distension in the upper and inner angle of the orbit, and it was feared that it might be a new growth. On opening externally, he found he could secure no passage into the nose. But he forced a passage and then introduced the rasp. Following the example of Dr. Watson-Williams, he used a backward-cutting punch, such as had long been used for the antrum, and found it acted extremely well in removing the crest on the posterior surface of the nasal bone, and with scarlet-red ointment the passage was kept smooth and free.

Mr. TILLEY said he would like to ask Dr. Watson-Williams to keep a very special record of the cases in which the inter-sinus septum had been perforated, in order to compare the results with those which had not thus been operated on. It was difficult to see how merely making a perforation between the two sinuses could expedite a rapid cure and a relief of symptoms. It was often one's experience to see pus return from one sinus when the other was irrigated. He had never been able to convince himself that the cases in which perforation had been caused by disease got better any quicker than did those in which the septum separated the two diseased sinuses. Only within the last two months he had had a patient in whom the sinuses communicated. There was a fronto-

nasal canal into both frontal sinuses almost big enongh to admit a cedar pencil, and on syringing up one the fluid flowed freely down the other. Nothing seemed to stop the suppuration, not even the employment of strong antiseptics. If one broke down the inter-sinus septum, one must chip off portions of bone, and he would like to know what happened to these. Did they get blown down, washed down, or did they slowly disintegrate within the sinus ?

Dr. WATSON-WILLIAMS replied that he remembered Dr. Dundas Grant showing Fletcher Ingals's instruments and burrs, as well as his own "sounds," and he (the speaker) had used Dr. Grant's sounds, and they were useful up to a certain point. But the difficulty was that they merely dilated by stretching the parts, which consequently fell together almost as soon as the sound was withdrawn. In reply to Mr. Tilley, he did not think there would be any more difficulty with these small pieces of bone from the septum than with those from the rasped ethmoidal cells. Either they remained attached to muco-periosteum and would not necrose or if loose they would become washed away. He agreed that if one got the frontal sinus so well open that a good flush could be procured by washing one sinus, there was no necessity for opening the septum. But both these cases had hung fire very much, although he had made a good opening into them ; in each case they had very much improved since the free openings had been made in the frontal sinus septum, allowing not only of a very free flush through, but also of better atmospheric ventilation.

Case of Laryngeal Disease of Obscure Nature.

By W. JOBSON HORNE, M.D.

THE patient, a man, aged 36, had had no throat affection until Christmas, 1913, when he had laryngitis; since then there had been hoarseness and almost loss of voice for five months. Three or four months ago there was dysphagia, when only slops could be taken. There is no history of lues or phthisis.

The tongue is protruded to the left. An overhanging epiglottis, which is normal, makes it difficult to see the vocal cords, which are almost hidden by the ventricular bands. There appears to be impaired mobility of the left cord.

On the one occasion the patient was seen by the exhibitor it was not possible to investigate the case further.

DISCUSSION.

Dr. GRANT said there was paralysis of the left vocal cord, and loss of function of the left hypoglossal nerve, while the pharyngeal branch of the vagus seemed to have escaped. If there were assumed to be a growth in the medulla, a remarkable point was that with paralysis of the left half of the tongue there seemed to be no wasting of the muscle, such as would ensue if it were due to infra-nuclear disease. The case seemed worthy of special neurological examination.

Dr. D. R. PATERSON thought the loss of voice was largely due to approximation of the ventricular bands, and the question arose whether anything should be done for that, as it considerably interfered with the vibration of the true cords. He had had experience of such cases, and they seemed to be particularly unsatisfactory where the false cords were much thickened. Painting them with astringents failed to yield good results. It seemed to him that this aspect of the case was well worthy of discussion.

Dr. FITZGERALD POWELL thought that in view of the paralysis of the tongue, central nervous trouble was to be feared, and he would suggest that the eyes should be examined and the opinion of a neurologist obtained.

Dr. JOBSON HORNE, in his reply, stated that the larynx had undergone considerable change since the one occasion upon which he had seen it. At that time, owing to the œdema of the ventricular bands, it was difficult to see the vocal cords. Rest of voice was enjoined, with the result that now it was possible not only to see the vocal cords, but also to see that there was ulceration on the glottic aspect of the left arytænoid region, perhaps more extensive than the mirror showed. Although there was no history of lues obtainable, he would nevertheless advise that a Wassermann test be done and the future treatment of the case be based upon the result.

Abnormal Overgrowth of Nose (Bulbous) in a Child, aged 2.

By J. L. IRWIN MOORE, M.B.

THIS case was sent to me at the London Throat Hospital. The child was born in this condition. Dr. Worthington, of Exeter, who was consulted, writes me as follows:—

"This case was brought to me about a year and a half ago with a tumour at the end of its nose much the same in appearance as it is now. I thought the swelling was cystic, and that it might be a dermoid in an unusual situation. I found on cutting on to it, however,

FIG. 1.

Abnormal overgrowth of nose (side view).

FIG. 2.

Abnormal overgrowth of nose (front view).

that it consisted of fine fibrous tissue, closely adherent to the skin and also to the perichondrium of the cartilage of the alæ. There was no capsule and no definite limitation whatever. I removed as much as I could in an attempt to at any rate make a more shapely nose. But it was a very difficult and unsatisfactory job, for the whole face was on a much smaller scale than it is now. The wound healed well and the appearance was certainly improved, but the tumour again slowly recurred until in proportion it was just the same as before. I have seen the case again off and on since last February, and I am sure that it has not grown any larger since then. After carefully considering all the possibilities, I have declined to operate again as, short of cutting off the end of the nose, I do not think removal is practicable, and partial removal my experience has shown to be of no avail. You will notice that the substance of the growth comes well on to the columella in front."

DISCUSSION.

The PRESIDENT said he would have thought the case might well have been operated upon again, and the tip of the nose considerably reduced. Mr. Graham had operated on a similar case, not so pronounced, with success.

Dr. JOBSON HORNE advocated waiting to see if the abnormal portion of the nose kept pace in growth with the rest of the nose. If it did not, in time the nose might outgrow the abnormality, and no operative interference for cosmetic or æsthetic purposes would be necessary.

Mr. HARMER said the case was an admirable one for diathermy, which could be performed without shock to the child, and without causing much scarring.

Dr. IRWIN MOORE agreed with Mr. Harmer's suggestion to try diathermy.

Male, aged 59, with Epithelioma of the Œsophagus, shown to illustrate the Benefits of Hill's Feeding Tube.

By J. L. IRWIN MOORE, M.B.

RADIOGRAMS by Dr. Finzi show the stricture and dilatation of the œsophagus above it. Patient had dysphagia for two months, and when first seen on September 18 he had lost 37 lb. in weight. Solid or semi-solid food if swallowed was soon vomited. He could only take liquid food. There was a large secretion of mucus day and night, of which he

vomited a breakfast-cupful at a time. The tube has remained in situ for seven weeks and food is retained. He can now swallow liquids by the mouth without discomfort. He is relieved of the mucous secretion. He is gaining in weight, is comfortable and in good spirits, and able to do his daily work.

DISCUSSION.

Dr. D. R. PATERSON asked whether these tubes had been found useful in malignant disease at the entrance of the œsophagus, where the difficulty caused by inability to swallow the saliva was very annoying.

Mr. ROSE asked whether these tubes had been used for cases of non-malignant stricture, and whether there was a tendency for such strictures to dilate whilst the tubes were being worn.

The PRESIDENT said that in his experience these styleted tubes were not well tolerated in all post-cricoidal cases. Symonds's soft rubber tubes should always be tried first in those cases—the long tube, not the funnel; but they were liable to be coughed up or vomited. That occurred in a case of his that very day and a styleted tube had to be substituted. He used these tubes for their bougie effect in cases of non-malignant stricture, and they usually acted very well. If not they could be dilated more drastically afterwards with a Brünings's dilator.

Dr. IRWIN MOORE replied that he thought the case showed the great advantage of feeding such cases by means of Dr. William Hill's oro-œsophageal tube, especially from the patient's point of view. When he first saw the patient on September 18 he had lost 37 lb., was very miserable from starvation, and practically ready for gastrostomy. He had had no food for four days, liquids, which he had previously been able to swallow, being vomited almost as soon as swallowed. He could now take 9 pints of food in twenty-four hours, consisting of beef-tea, milk, custards, Benger's food, and light boiled eggs—a considerable portion of which (during the past two weeks) he had been able to swallow by the mouth, and alongside of the tube. He had put on 6 lb. in weight in seven weeks.

Perithelioma of the Pharynx.

By J. COUBRO POTTER, M.D.

DR. POTTER said that the case was referred from the May meeting for a report.[1] The diagnosis had been confirmed by Mr. Shattock, as follows:—

[1] *Proceedings*, 1914, vii, p. 187.

Report upon a Nasopharyngeal Tumour referred for opinion from the Section.—The neoplasm consists of groups of tortuous, closely packed, well-defined columns of cells. Many of these exhibit a lumen, which is in some cases empty, and in others holds a finely granular or homogeneous coagulum, which has slightly shrunken from the containing space. Some of the channels are wide and complex. The lumen of the different spaces is lined with a flat-celled endothelium, the cells on the outer aspect of which are cuboidal or columnar. Channels with similar structureless contents occur in certain areas of the connective tissue, but furnished only with a flat endothelium; this indicates that the flattened lining elsewhere is not the result of pressure arising from the central formation of a degeneration product, but represents a proper endothelium. The new growth falls into the class of perithelioma, the channels being, presumably, of lymphatic origin.

July, 1914. S. G. Shattock.

Brain Abscess secondary to Frontal Sinus Suppuration; Drainage; Recovery.

By Dan McKenzie, M.D.

The patient, a young man, aged 27, was brought to the Central Throat and Ear Hospital by Dr. Battersby Jobson with a fistula in the right eyebrow leading into the frontal sinus of that side.

The history was as follows: Six weeks before, the patient, who had for long been the subject of a purulent discharge from the nose, suddenly developed an abscess in the right upper orbital region with high fever and some delirium. After the abscess had been freely opened by Dr. Jobson, however, these constitutional symptoms rapidly disappeared and the patient got well enough to go to business, to frequent football matches and to lead an altogether normal life. As the fistula did not close, however, he was brought to hospital.

An X-ray examination having been made according to routine, the patient was admitted for operation. The X-ray plate on this occasion showed a "normal," that is, an air-filled sinus. After he had been put under chloroform I inserted a probe into the frontal sinus through the fistulous opening and was surprised to find that it passed an unusually long way into the cavity of the skull, so that it seemed as if it were an enormous frontal sinus we had to deal with. But on opening up the

sinus we found it to be quite a small one, with a second fistulous opening in its posterior wall leading to a large abscess, seemingly within the frontal lobe. About 6 dr. of fetid pus was evacuated. The fistula having been slightly enlarged, a drainage-tube was placed in the abscess cavity with its outer end projecting from the supra-orbital wound. The

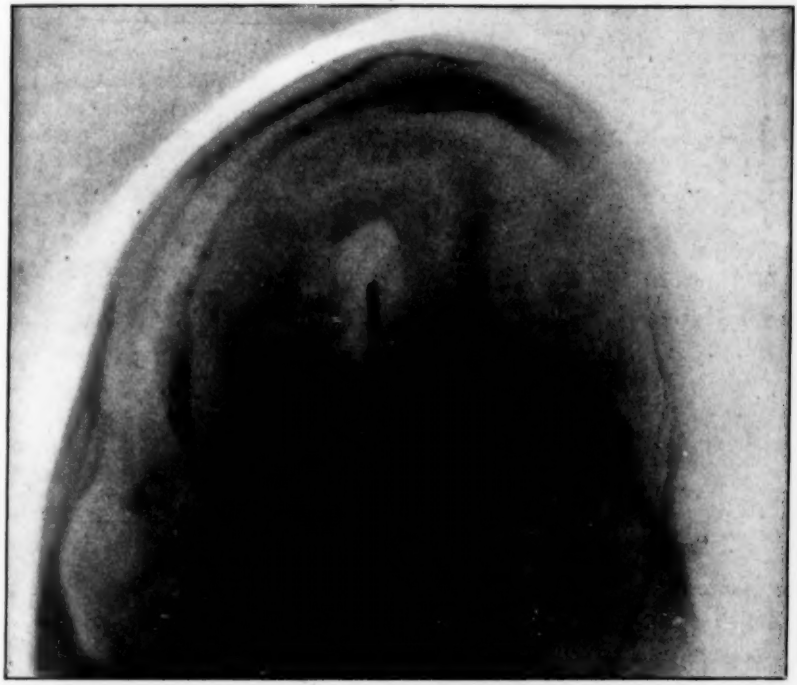


FIG. 1.

Abscess of frontal lobe. Probe in abscess cavity inserted through the frontal sinus. (Antero-posterior view.)

débris removed from the abscess was reported by Dr. Wyatt Wingrave to contain cerebral cortical tissue.

The patient did very well. No untoward symptoms made their appearance, and, a month later, being undecided as to how long the drainage-tube, which I had meanwhile shortened a little, should be retained, I re-opened the sinus to find the brain abscess cavity apparently obliterated and the tube lying amid granulations. It was therefore

removed, and, after another tube had been inserted through the infundibulum and out at the anterior nares so as to drain the sinus, the forehead wound was entirely sutured. Six weeks later the maxillary antrum, which was also the seat of suppuration, was opened and drained. The patient now seems to be quite well.

FIG. 2.

Abscess of frontal lobe. Probe in abscess cavity, inserted through the frontal sinus. (Lateral view.)

The only symptom which might have been due to the brain abscess was a rather low pulse-rate, about 60. There was no appreciable interference with the intellectual powers; no changes in the optic disk; no emotional disturbances, and no paretic or paralytic phenomena.

X-ray plates (by Dr. Ironside Bruce), showing a probe in the abscess cavity, were on exhibition (figs. 1 and 2).

DISCUSSION.

Mr. HORSFORD said that he operated upon a case a year ago for extensive nasal suppuration, double antral and ethmoidal. About three weeks ago he was asked to see the patient again. Recently he had had frontal and occipital headache, disturbance of gait, and a tendency to fall forward. He found that the left sphenoidal sinus was diseased, and he opened it freely. There was relief of the posterior headache, but the other symptoms persisted. The right sphenoidal sinus was also diseased. He had slow cerebration, a tendency to fall forward and latterly to fall backward, indefinite headache, a pulse-rate of only 50 to 66, a subnormal temperature, and, in fact, most of the symptoms pointing to cerebral disease. Dr. Risien Russell thought there were definite, though slight, signs pointing to frontal abscess. Mr. Donald Armour explored the skull in that region, and opened the frontal sinus on that side, but found but little disease of the frontal sinus and no frontal abscess. Nothing further was done, and the patient died next day. Post mortem there was found an extradural effusion of clear fluid in two places in the anterior fossa on the right side, and nothing else abnormal. This case showed the difficulty in the diagnosis of cerebral complications of nasal disease.

Major P. G. GOLDSMITH (Canadian Army Medical Corps) desired to congratulate Dr. McKenzie on his case, which was well worked out. The result drew one's attention to the fact that suppuration might exist in the frontal sinus with but few symptoms, especially if there were no meningitis. He saw a case in which the patient died of acute osteo-myelitis of the frontal bone with orbital phlegmon following a radical frontal sinus operation. There was found post mortem an abscess larger than a walnut, with thick walls. There had been only slight headache, no nausea, nor optic neuritis, nor was there any apparent communication between the suppurating sinus and the dura or abscess.

Sir STCLAIR THOMSON pointed out that the condition was not due to operation, and for their own sakes members should bear these things in mind. He had a case in hospital in which he operated upon a frontal sinus, and it appeared to be doing well. But the patient then developed symptoms pointing to an abscess in the frontal lobe on the opposite side. A post-mortem revealed a lateral abscess in the brain on the untouched side. In private practice it was possible that the surgeon might be blamed for what was in no sense due to operative traumatism.

Aspergillosis of the Maxillary Antrum.

By Herbert Tilley, F.R.C.S.

(*With Histological Report by* S. G. Shattock, F.R.C.S.)

Five microscopic specimens and drawings are shown to illustrate:—

(1) A section showing the general structure of the antral contents.

(2) Mycelium in section.

(3) Octahedral crystals.

(4) Aspergillus mycelium in order to contrast it with

(5) "Thrush" mycelium.

I have met with five cases. The first case was described in the *Lancet*, July 17, 1909, under the title, "Case of Endothelioma Myxomatodes of the Maxillary Antrum."[1] This title was suggested by Mr. S. G. Shattock, who had made sections of material removed from the antrum, and his description and illustration (drawn by Mr. Ford) held good for the specimens from a later case which have been shown to-day. Careful study of a more recent case has shown that while the clinical symptoms and main histological features are the same in both, yet the fact that the aspergillus mycelium has now been identified in the specimens derived from *each* patient renders it necessary for us to delete the older diagnosis and to replace it by what we venture to name "Aspergillosis of the Maxillary Antrum." Mr. Harmer also reported a case last year.[2]

My cases all occurred in adult females: in two patients both antra were affected, and in one of these the ethmoidal cells on each side were infiltrated with the same characteristic material.

The symptoms of this condition are: Persistent discharge of mucus or muco-pus; fits of violent sneezing with the occasional expulsion of greyish-white, viscous masses; headache, with neuralgic pains in and around the cheek and eye. In my first case these last-named symptoms were so severe that sleep was often impossible, and the life of the patient rendered miserable. The nasal mucosa is generally pale and œdematous, and strong solutions of cocaine have little effect

[1] *Lancet*, 1909, ii, p. 136.

[2] *Journ. of Laryng.*, 1913, xxviii, p. 494.

in reducing this condition. In two patients the inner antral wall immediately above the inferior turbinal was bulged inwards, so as to suggest that the sinus was occupied by a large cyst. Transillumination revealed marked opacity of the antrum, and it was impossible to obtain a return of the fluid when irrigation was attempted through an exploring trochar.

I operated on each case through the canine fossa. On removal of the bone, one is at once struck by the bluish-grey, glistening surface of the antral contents and the absence of bleeding. The material is easily separated from the walls of the sinus ; its consistence and appearance are remarkably like the contents of a muscatel raisin with the stones or pips removed, so viscous is it that it is easier to scrub than to wipe it from a curette or spoon. In each instance the Caldwell-Spicer-Luc operation was performed, and so far I have seen no recurrence.

HISTOLOGICAL REPORT UPON THE MATERIAL REMOVED FROM THE ANTRUM BY MR. HERBERT TILLEY, FEBRUARY, 1914; BY PROFESSOR S. G. SHATTOCK.

The material consisted of a flattened, irregularly oval mass 2 cm. in longer diameter and 7 mm. in thickness, of firm consistence, homogeneous, and somewhat translucent. The most successful microscopic sections of it were those made (after formol hardening) by means of the freezing microtome, on which it cut like firm cheese; its peculiar consistence rendered it particularly intractable in paraffin. The sections were stained by the various methods likely to throw light on its structure; they were all made so as to include the whole of the transverse area of the mass. They show two elements—viz., a mucinous basis, and cells variously disposed in it, without fibre or vessels. In some situations extensive areas of the homogeneous matrix occur in which spheroidal cells are thickly scattered; in others the cells are massed in groups of varying size, the mucin around which forms trabeculæ wherein cells are sparsely distributed, or in which they are arranged in narrow lines which pass by gradations into the more voluminous groups. Cell necrosis has occurred in many situations, chiefly in connexion with the elements distributed in the substance of the mucin, as distinguished from those which compose the groups.

A study of the cells first mentioned shows that they are spherical

in form, and mostly present the subdivided nuclei of polymorphonuclear leucocytes. In regard to the loculated areas the cells in these are, as already noticed, arranged in well-defined groups, the mucinous matrix forming a stroma, as in a neoplasm of alveolar type, in which the connective tissue has undergone complete mucinoid degeneration. The mucinous substance bounding the groups is in places fibrillated in correspondence with the contour of the latter. The cell nuclei of the groups are of very diverse figure, many being flattened and angular, and the whole appearance offers a remarkable simulation of such a

Fig. 1.

Section of the mucinous material from Mr. Tilley's first case, showing the long collections of cells lying in its fissures and simulating an endothelioma with mucinoid degeneration of its stroma. The magnification of the right half is low ($\frac{2}{3}$), that of the left half is higher ($\frac{1}{2}$). The general construction is similar to that described in the present communication.

tumour as an endothelioma of a salivary gland. A close study, nevertheless, leads to the conclusion that the cells in question are polymorphonuclear leucocytes which have become flattened, elongated, and distorted by pressure, since in hæmatoxylin-eosin preparations the cytoplasm exhibits the fine eosin-staining granules of this class of cell; and, further, in the smaller of such groups polymorphonuclear leucocytes of normal characters are present, and from these the other

forms can be traced. In sections, moreover, simply treated with liquor potassæ, the presence of fine discrete granules is sharply detailed in the cytoplasm of the flattened cells, as it is in the polymorphonuclear leucocytes elsewhere.

The last elements to notice (excluding the micro-organisms) are coarse crystals which occur in the mucin in conspicuous numbers. They vary in size, and in places lie in parallel groups. They are fusiform, although some present an obtuse angle on each side at the middle; they are liable to transverse fracture, and in cross-section are, without exception, regularly hexagonal. They are tinted with the eosin

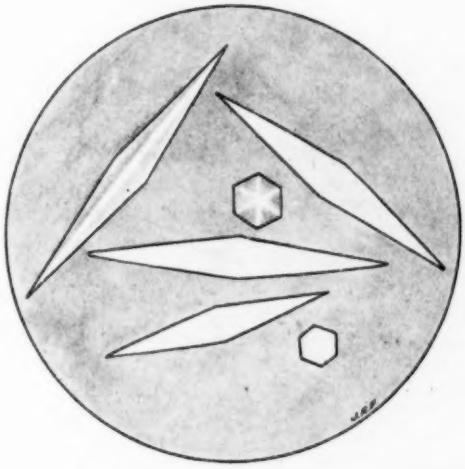


FIG. 2.

Samples of the crystals present in the sections made from the material removed from the antrum. In cross-section they are regularly hexagonal. ($\frac{1}{8}$ obj.)

used after hæmatoxylin: coloured blue with carbol thionine, and pale reddish brown with 1 or 2 per cent. aqueous solution of iodine in iodide of potassium, the surrounding mucin becoming yellow. They are not affected by 25 per cent. sulphuric acid, nor by liquor potassæ (British Pharmacopœia). They are not dissolved in sections incubated for twenty-four hours at 37° C. in distilled water, to which a trace of ammonia has been added. It may be noted that these crystals lie at times between cells where there is no intervening mucin.

An examination of unstained sections, mounted in dilute glycerine, with the polariscope shows that the crystals are doubly refractive; with crossed Nicols they stand out brilliantly on the dark background. These crystals have not yet been identified. When viewed laterally they resemble the Charcot-Leyden crystals found in asthmatic sputum, and those of Schreiner's spermin phosphate obtained from semen[1]; they are, however, regularly hexagonal in cross-section, whilst the Charcot crystal is an elongated octahedron.

Whether or not we are dealing with a single substance, crystallising dimorphically, cannot be determined from the data available, the value

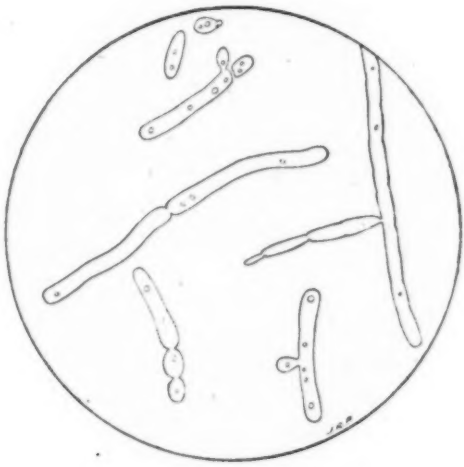


FIG. 3.

Blastomyces albicans (thrush); living, in a hanging drop; showing the constriction accompanying the process of budding. (No details of structure are shown in this or in figs. 4 and 5.) ($\frac{1}{12}$ obj.)

of which is somewhat reduced, moreover, by the fact that the crystals in the specimen are not chemically pure (having been formed in an organic fluid), and that the material itself had been fixed in formalin.

In certain areas the mucin is pervaded with a well-developed mycelium, the coarseness of the filaments of which at once excludes any form of actinomyces. When in an intact or healthy condition it

[1] The identity of Charcot's crystals with spermin phosphate is not certain, although often assumed. (George Barger, "The Simpler Natural Bases," 1914.)

constitutes a striking feature; but a close study shows that its extent is much wider than at first appears, for as it dies its position is indicated only by shrunken fragments, or narrow lines of fine granules, or merely by tubular spaces which, though empty, reproduce the form of the vanished organism. Indications, some or other, of its presence occur in almost every part of the section, except, perhaps, in the circumscribed groups of cells which occupy the cavities or fissures in the mucin. In addition to this mycelium, there are, needless to say, collections of bacteria, but these are much less widely distributed than might have been expected.

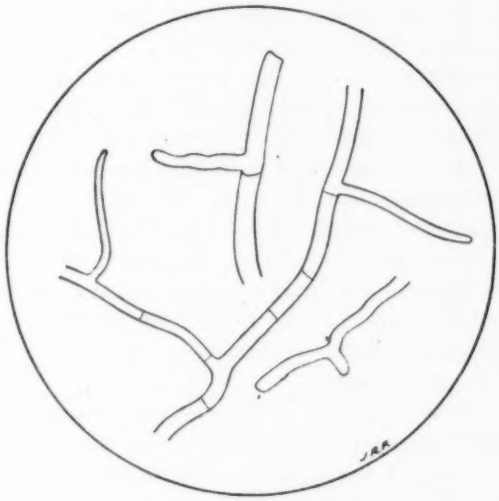


FIG. 4.

Portions of mycelium of *Aspergillus niger*, showing lateral branching, the offsets being cylindrical and of uniform diameter from the first; and the septa of the filaments being rectilinear. In this and the following figure, as the sole object is to show the method of branching, portions of the mycelium have been selected and drawn in an isolated or fragmentary manner. (⅛ obj.)

As regards the identification of the mycelium, it may be differentiated from that of *Blastomyces albicans* (thrush) by its mode of extension, which takes place not by a process of constricted budding, but by the simple lateral extrusion of short processes which are of uniform diameter from the outset. In the mycelium of blastomyces the septa, moreover, between the segments (which segments result from

the repeated formation of a terminal bud), are biconvex and marked externally by a slight recession of contour. The mycelium of aspergillus, on the contrary, presents septa which are rectilinear and rectangular; and its lateral buds, at first hemispherical, afterwards elongate without deviations from the cylindrical form.

There is a third organism which needs referring to—viz., sporothrix. In artificial culture this produces a branching mycelium (coarser than that of actinomyces), the lateral off-shoots of which are of uniform diameter; but, in addition to this, ovoidal yeast-like "spore-bodies" bud out from the sides of the mycelial threads. That the latter are not simple mycelial extensions appears from the fact that when the mycelium dies in old cultures these other structures persist, and on subculture reproduce the filamentous forms.[1] Mr. A. G. R. Foulerton points out, however, that in the lesions produced by *Sporothrix Schenkii*, whether natural or artificial, mycelium is not developed, but the ovoid cell-forms or "spore-bodies" exclusively. The human lesions produced by Schenk's sporothrix, as hitherto observed, have been refractory subcutaneous abscesses. On the other hand, Pinoy[2] has described lesions in the human subject caused by *Sporothrix Beurmanni* (ulcerating granulomata of the skin, pharynx and larynx), where a mycelium developed in the living tissues, around capillaries which became perforated, and within which the organism produced its "spores." In these different instances, however, "yeast-forms" predominated in the tissues.

Summary.—Closely as some of the microscopic appearances simulate an alveolar type of tumour, of which the stroma has undergone mucinoid degeneration, it seems more correct to regard the formation as a pseudoplasm somewhat akin to the pseudomyxoma of the abdomen which results from the effusion of mucin from a distended appendix, or of the mucinoid contents of an ovarian cyst into the peritoneal cavity. The simulation was so great in the first specimen which Mr. Tilley handed over to me, that I then ventured to suggest the name "endothelioma myxomatodes" for the lesion. A re-examination of the microscopic sections from this case shows the presence of mycelium in the mucinous matrix, though it is recognisable in only a few spots. The formation is best viewed as consisting of mucin

[1] A. G. R. Foulerton, *Trans. Path. Soc.*, 1901, lii, p. 259; and *Brit. Med. Journ.*, 1912, i, p. 301.

[2] *Compt. rend. de l'Acad. des Sciences*, Par., 1911, clii, p. 286.

secreted by the mucosa under irritation, and subsequently inspissated, the fissures or less resisting lines of which have been invaded with polymorphonuclear leucocytes, as in the first stage of the organisation of a blood-clot; in this way the collection of cells in elongated groups may be explained. It contains no fibre and no capillaries, although, as its removal was clinically accompanied with slight hæmorrhage, its area of attachment consisted, probably, of vascular granulation tissue. The contained cells are polymorphonuclear leucocytes, the nuclei of which in the circumscribed cell groups are mostly so flattened as to

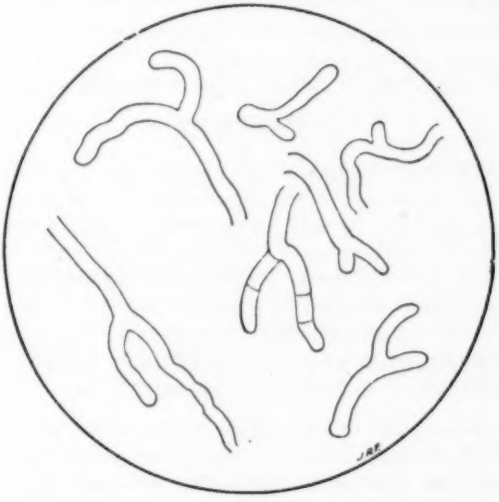


FIG. 5.

Portions of the mycelium in the material from the antrum, showing a mode of branching as in aspergillus. The organism is much coarser than any form of actinomyces. ($\frac{1}{11}$ obj.)

suggest an endothelial origin; but their finely granular cytoplasm, the granules of which are eosinophile in hæmatoxylin-eosin preparations, indicates that the cells are distorted leucocytes.

In regard to its pathogenesis: the disease may provisionally be attributed to the growth of the mycelium, although the proof of this would in a future case rest upon the culture of the micro-organism, and the experimental production of a similar lesion by direct infection of the maxillary sinus in one of the lower animals. On this assumption

the disease would be a mycosis of the mucosal surface. The fact that well-conditioned mycelium occurs in areas of leucocytes apart from the presence of mucin militates, amongst other things, against the supposition that the organism is the source of the matrix. A comparison of the mycelium with that of *Actinomyces*, *Blastomyces albicans*, and of *Aspergillus*, brings out its morphological likeness to to the last named; whilst the rarity with which sporothrix produces mycelium in the living body, and the fact that when this does occur the mycelium is accompanied by the presence of ovoidal yeast-like "spore-bodies," would exclude a sporotrichosis. Such a view of its mycotic pathogenesis harmonises, also, with the observation that in certain of the cases the disease has in its course affected not only the antrum but parts of the nasal cavity.

DISCUSSION.

Mr. HERBERT TILLEY added that the Section should be much indebted to Mr. Shattock for having elucidated the nature of this material taken from the series of cases: for he had spent many hours—probably a total of weeks—in unravelling the nature of this mysterious material removed from the antra of the speaker's cases. The specimens shown in connexion with the cases were models of what microscopic specimens should be. There was no dilation of the antral walls in any case; there was some mucopurulent nasal discharge in most of the cases, but nothing to be compared with the suppuration of an ordinary antral empyema.

Mr. HARMER said that in his case the fungus could be grown, and it was found to be typical *Aspergillus fumigatus*. The symptoms in his case were very similar to those described by Mr. Tilley, but there had been great difficulty in curing them; ordinary drainage and washing out of the antrum was useless. In his own case probably the ethmoidal region also was involved. The symptoms could be controlled by giving large doses of iodide of potassium, but the fungus tended to recur, and the case was still uncured, though treatment commenced eighteen months ago.

The PRESIDENT expressed the gratitude of the Section for Mr. Tilley's clear description of the clinical symptoms, and for Mr. Shattock's lucid explanation of the pathology of this newly described disease, which might be designated morbus Tilleyi. In reference to the statement that potassium iodide sometimes had a favourable effect on this condition, it was to be noted that in an allied mycotic disease, actinomycosis, it had been claimed that iodides sometimes acted so well that radical surgical measures became unnecessary.

Dr. D. R. PATERSON said he had known a case where an aspergillus was found in the contents of the lachrymal sac. It was thought to have entered

through the nose by the lachrymal duct and established itself upon a condition which had existed for some time. He wondered whether, in Mr. Tilley's cases, the aspergillus was grafted on to some old suppurative condition.

Mr. TILLEY replied that he was unable to answer Dr. Paterson's question. He could get nothing but an indefinite history of pain, sneezing, &c. He thought the aspergillus gained an entry to the nose and sinuses and caused the symptoms, rather than that it was grafted on to a chronic empyema. It was a point which might be settled in some future cases.

Mr. SHATTOCK replied that it was quite possible, of course, that an aspergillary infection might be grafted upon a commoner suppurative condition—i.e., that the infection might be mixed, as it might be in cases of pulmonary aspergillosis.

Intrinsic Epithelioma of the Larynx, suitable for Laryngo-fissure.

By Sir STCLAIR THOMSON, M.D.

THE patient, aged 69, complained of absolutely nothing except hoarseness, which has been coming on for six months. The whole of the left vocal cord is replaced by a red, knobby, ulcerating infiltration; the cords move well; the rest of the larynx is quite normal; there are no enlarged glands. The Wassermann reaction is negative. Patient weighs 13 st. 6 lb. There are no indications of tubercle. He is a non-smoker.

The case is shown to illustrate the difficulty of diagnosing certain cases of epithelioma of the vocal cord. The infiltration is not suitable for removing a portion for microscopic examination, and the diagnosis therefore depends entirely on the naked-eye appearances and on exclusion of other possibilities.

It is proposed to treat the case by laryngo-fissure, for which it seems eminently suitable.

DISCUSSION.

Mr. HERBERT TILLEY said he did not think there was any doubt about the diagnosis. It was one of the clearest cases showing freedom of the cord in malignant disease which he had seen.

Sir FELIX SEMON said that he regretted he had not seen the case himself, but as it was stated that the whole of the left vocal cord was replaced by a red knobby ulceration, he thought that operation should not be delayed

longer, as this could only increase the gravity of the operation. At the same time, hearing that the mobility of the cord, in spite of the extent of the disease, was quite unimpaired, he wondered whether *senile tuberculosis* was altogether excluded. He also was reminded of certain odd cases which Mr. Shattock and he had seen together, in which, clinically, there could apparently be no doubt as to the malignancy of the condition, whilst microscopically the affection was found to be an obscure infective inflammation, which even Mr. Shattock did not know how to describe exactly. Thus this case, too, after all, might not be malignant. All the same he agreed with Sir StClair Thomson's operative proposals.

Dr. DUNDAS GRANT advised Sir StClair Thomson not to remove a large portion of it by means of Jurasz's forceps. He once did so in a similar case which he believed to be malignant, and he pulled away nearly the whole of the vocal cord, and it proved to be non-malignant. The patient's anxiety was removed but the voice was not recovered. He did not regard the present case as certainly malignant. Mobility of the cord did not exclude a diagnosis of malignancy, but mobility would be most unusual with a growth of this extent if malignant. He would be much interested in the result of the microscopical examination of a small fragment.

Dr. DAN MCKENZIE asked why the exhibitor had thought it an unsuitable case for the removal of a portion for microscopical examination. In supposed laryngeal cancer, the absence of any support of the diagnosis from the microscope would lead the speaker to hesitate to perform thyrotomy.

Dr. JOBSON HORNE agreed that the case was not very obviously a case of epithelioma of the vocal cord; the cord was so remarkably free in movement in spite of the extent of the ulceration. He had seen similar cases which subsequently were proved to be senile tuberculosis of the larynx.

Dr. FITZGERALD POWELL said that he entirely agreed with Sir StClair Thomson as to the nature of the cases and as to the procedure he proposed to adopt. He could not agree to the suggestion of punching out a portion of the diseased cord for diagnosis. If the disease was tubercular it was better away, and if malignant, as he thought it was, it would only hasten the spread of the growth from the cord to the deeper structures.

Sir STCLAIR THOMSON replied that the patient's blood-pressure was 200 and his age 69, so that it was not such a very simple case to operate on. He showed the case because in some instances one had to wait for some indication of malignancy. This was a case in which the "penny-in-the-slot" method of diagnosis did not work; it depended entirely on a trained eye. He did not regard it as an absolutely clear case, but he brought it to show how one must sometimes act on a probability. With regard to tubercle, the patient had been overhauled by a physician. With regard to removing a portion for examination, this was not a growth "on" the cord. Sir Felix Semon would agree that

there were cases in which the cord was infiltrated and there was no differentiation between the cord and the growth. If a portion were removed and reported to be non-malignant, he would still not like to leave that patient alone. If it were his own larynx, he would not have it touched for removal of a piece, because of the danger of rapid spread afterwards. Senile tuberculosis of the larynx was very difficult to diagnose, and it would often occur in the absence of pulmonary or other symptoms. One thing he did as a routine, which was helpful when doing laryngo-fissure: When the larynx was split, instead of having a pathologist to examine the growth on the spot, he felt it with the finger; there was a peculiar cartilaginous feel which he regarded as almost pathognomonic.

Cases of Pulsating Elongated Swelling on Lateral Wall of Pharynx.

By W. STUART-LOW, F.R.C.S.

Case I.—A boy, aged 7, who has a pulsating elongated swelling on the lateral wall of the pharynx on the left side just behind the tonsil. He was sent to the hospital for removal of tonsils, no complaint being made of any inconvenience in the throat. The interesting points with reference to this case are: The youth of the patient, the danger of injury to the vessel in operating on the tonsils, and whether it is likely to develop into the same troublesome and sometimes painful condition—that of the second case.

Case II.—A man, aged 60, with a similar pulsating swelling also on the left side of the pharynx; the pulsation can be felt up the side of the neck towards the ear. He complains of a throbbing in the neck, and sometimes of pain, and that the beating keeps him awake at night. This is most likely an aberrant branch of the carotid or the ascending pharyngeal artery; pulsations can be both seen and felt in the pharynx and neck. If the boy shows an early stage of what the man illustrates, can anything be suggested to prevent him developing what is an incapacitating condition?

DISCUSSION.

Dr. PETERS thought Mr. Stuart-Low might safely perform tonsillectomy, and might use the new guillotine exhibited that afternoon. The second case he regarded as one of arterio-sclerosis: that the symptoms were due to degenerated arteries, rather than to one particularly enlarged vessel.

Mr. HERBERT TILLEY said he had had a case identical with the first of these, and because of the marked degree of pulsation he postponed operation on a large septic tonsil, but in six months the symptoms increased so much that the operation was decided on, and all preparations were made for severe hæmorrhage, but he did not think he had ever seen a case in which tonsillectomy was accompanied by less hæmorrhage.

Dr. GRANT desired to congratulate Mr. Stuart-Low on the sharpness of his vision. Perhaps if this condition were more frequently sought for it would be oftener found. He did not think there was any reason for anxiety that it would develop into a troublesome painful condition. With regard to the request for suggestions to prevent the case developing into an incapacitating condition, there were some enlarged and infiltrating glands which seemed to push the vessel a little farther into the pharynx than would be the case in their absence. In the second case he thought there was a slight drooping of the left eyelid, suggesting interference with the sympathetic, producing weakness of the non-striated muscle in the orbit.

Dr. D. R. PATERSON said that in the second case there was deafness and tinnitus aurium present which added to the discomfort, and thought the pharyngeal condition might have but little to do with his symptoms. He had certainly seen just as prominent vessels with no complaint whatever on the part of the patient.

Sir FELIX SEMON said that a perusal of the *Transactions* of the old Laryngological Society would show the records of several such cases of pulsating vessels on the posterior wall of the pharynx.

Mr. STUART-LOW replied that he did not claim to possess exceptional acuity of vision. The discovery was due to his assistant Dr. MacKeith, who saw the condition when about to operate. The man was incapacited, as this kept him from sleeping and was costing the State money every week.

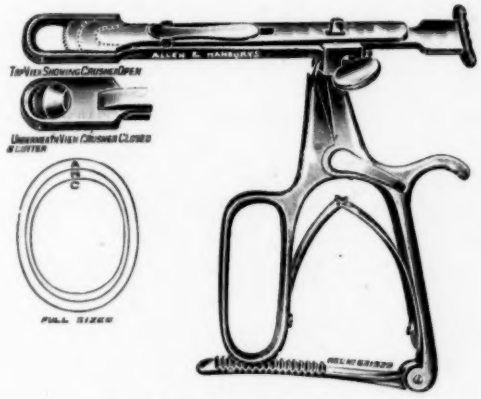
Hæmostatic Guillotine.

Shown by G. J. F. ELPHICK.

THE instrument was designed for the complete enucleation of tonsils, which are removed from their beds, with their capsules, without loss of blood.

The method used is approximately that of Sluder, but, in addition to the one cutting blade, there is a crushing blade, which enters between the anterior faucial pillar and the tonsil, and effectually crushes all vessels between the capsule of the tonsil and its bed. When the

crushing blade has been pushed home, it remains locked in this position by the Hagedorn catch at the base of the handle of the instrument. A light pair of fixation forceps is now applied to the protruding tonsil (i.e., on the uvula side of the blade), and the cutting blade, which enters between the crushing blade and the tonsil, pushed home. The tonsil is now lifted out on the fixation forceps. The crushing blade, being still applied to the vessels which have been cut through, may be left on for a few moments, and then gradually released by pressing down the catch with the little finger.



Hæmostatic guillotine.

The method is a simple one; there is no bruising of the faucial pillars or surrounding tissues, no muscle tissue is removed, and in vigorous adults where the risk of hæmorrhage may be great, it offers the safest possible way of preventing bleeding, in that all vessels that have been cut through can be held just as long as the operator pleases, and when released the proximal ends have been thoroughly crushed by the serrated edge of the crushing blade. The blades of the instrument are made in three sizes, and the one chosen should be of such a size that the tonsil can be just comfortably pushed through the ring. The fixation forceps are only necessary to prevent the tonsil from falling down when isolated.

DISCUSSION.

The PRESIDENT, referring to the statement on the agenda paper that Sluder's method was used, said that the technique carried out in this country was really that of Whillis (of Newcastle). It was a development of an older method which was illustrated in Sir StClair Thomson's book, in which enucleation was performed with the shaft of the guillotine lying across the buccal cavity. That Dr. Elphick's hæmostatic guillotine was an excellent enucleating instrument he could testify from personal experience.

Dr. DUNDAS GRANT asked how the cutting blade was to be driven, seeing that both hands were supposed to be already occupied.

Dr. ELPHICK replied that after pushing the tonsil through the ring with the left index-finger the crushing blade was pushed home with the right hand and automatically held in position by a Hagedorn catch, and both hands could be then released, if necessary, for pushing home the cutting blade.

Case of Deficient Tension of Vocal Cords.

By H. LAMBERT LACK, M.D.

THE patient is a man, aged 25, who has suffered from weakness of the voice three years or more. He cannot tell when it commenced but says he had a good voice when he was 18. There is no definite history of onset or cause. He has had a good deal of treatment without effect. On phonation there is an elliptical opening between the cords. The cords themselves are somewhat congested. There does not appear to be much functional element in the case.

Suggestions as to the pathology of the condition and treatment would be welcomed.

DISCUSSION.

Mr. HERBERT TILLEY said he thought it was a case of weakness of the internal tensors. He had never seen treatment do good in chronic cases, and he did not think this patient would get any better.

Dr. DUNDAS GRANT said it was interesting that this man practised the alto voice and sang in that voice in a choir. This meant some unnatural action of the muscles of the larynx; at any rate, it put a great strain on the fibres of the internal thyro-arytænoid muscles required for thinning of the cords and damping of certain portions of the cord. He saw this condition often, in minor degrees, as the result of laryngitis.

Case of Extensive Papilloma of the Trachea.

By H. Lambert Lack, M.D.

This patient was first seen in 1906. She had consulted various physicians and specialists, complaining of difficulty in breathing, which by some was considered to be asthma. A correct diagnosis had never been made until one day the patient expectorated a small piece of papilloma. When I first saw the patient she had considerable dyspnœa and pink granulations could just be seen in the lowest part of the trachea. The bronchoscope was not then in use. As the patient's condition was desperate I decided to give her chloroform, open the trachea as widely as possible and get the growths out quickly. This was done. The patient became more and more cyanosed as the operation proceeded, but directly I had pushed my finger down the trachea, breaking down large masses of papillomatous growths which completely obstructed the tube, the patient was able to cough up blood and growth and at once breathed more freely. A large amount of growth was then removed by forceps and curetting. The masses seemed to grow from all round the trachea for a distance of an inch and a half and more, extending nearly to the bifurcation. The patient made a good recovery, but the trachea was kept open for subsequent examination. Two or three weeks later some small growths were removed under cocaine and then the tracheal opening was allowed to close. The patient has remained perfectly well ever since. Lately, some small papillomatous growths have appeared on the skin of the neck. The case as far as I know is unique.

Dr. Grant said he had described a similar case.[1]

Epithelioma of Right Tonsil.

By Hunter Tod, F.R.C.S.

The patient, a man, aged 42, has noticed, for two or three months, slight pain on swallowing and shooting pains up to the ear on the affected side. On examination the right tonsil is found to be

[1] *Journ. of Laryng.*, 1904, xix, p. 649.

superficially ulcerated. The edge of the ulcer extends into the anterior and posterior pillars of the fauces and on to the base of the tongue. The margin is raised, everted, and indurated. No glands can be felt in the neck. It is a comparatively early case, and, in Mr. Tod's opinion, suitable for operation with the knife or for diathermic treatment.

DISCUSSION.

Mr. HARMER thought it was an early epithelioma and suitable for treatment by diathermy. The important question which was brought up at Aberdeen by Dr. Lack, as to whether diathermy was superior to the knife in the treament of all accessible growths of mucous membrane of pharynx and mouth, was raised by this case. He (the speaker) thought the results of diathermy were so admirable that it was preferable to a cutting operation in such a case. After diathermy the patient would be able to leave his bed on the second, or at worst the third day, and that could not be said after an even trifling operation with the knife.

The PRESIDENT thought the pronouncement made independently by Dr. Lack, Mr. Harmer and himself, was a very important one—viz., that it was permissible in the first instance, to try a non-cutting operation such as diathermy in throat cancer; with it there was less bleeding and less risk of implantation and dissemination, as the lymphatics were sealed. The results of surgical diathermy were very extraordinary, especially in a condition like advanced throat cancer, which it robbed of many of its terrors in the direction of obstruction of the upper respiratory and food passages.

Mr. TOD replied that he favoured diathermy and that his chief reason for showing the case was to get the opinion of the Section as to whether diathermy should be employed at once in preference to an operation with the knife.

Carcinoma of Palate.

By HUNTER TOD, F.R.C.S.

THIS case had been treated for lupus for over a year without success. A portion of the tissue was then examined by microscope and found to be carcinomatous. Treatment by diathermy, two sittings: (1) August, 1914, and (2) early in October. Growth now eradicated, except over small area on left side. Further diathermic treatment was proposed.

Ulceration of Soft Palate and Tonsil.

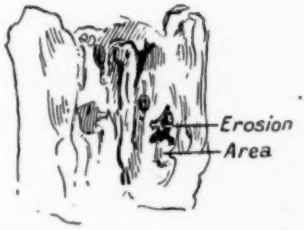
By WALTER HOWARTH, F.R.C.S.

PATIENT, a male, aged 12. The condition has lasted three months, and is said to have been progressive during that time, but is stationary at present. Section shows general hyperplasia, but no evidence of tuberculosis.

Meningitis due to Absorption of the Roof of the Ethmoidal Cells on the Right Side corresponding to the Posterior Half of the Crista Galli.

By P. WATSON-WILLIAMS, M.D.

A. S., ADMITTED on September 22, 1914, had been complaining of discharge from the nose and headaches for some months. The left and



Sketch showing the crista galli (in median line) and, on either side of it, the cribriform plate and other parts of anterior fossa forming roof of nasal passages. On the right side the erosion area is indicated.

right antra were explored and the left contained pus. Right antrum opened some months previously.

September 30, 1914: The left maxillary antrum opened intranasally. Frontal sinus not operated on.

October 1: Restless, complaining of pain behind nose.

Two days later developed symptoms of meningitis. F. O.: Right blurred and left same; optic neuritis.

October 4: Died with symptoms of general meningitis.

Post-mortem examination: Acute purulent meningitis originating from an area of erosion in the middle ethmoidal cells roof—i.e., opposite side to operation.

N.B.—(1) There was a normal left frontal sinus, but no right frontal sinus could be found; (2) if a right per-nasal frontal sinus operation had been attempted (as had been the case some months previously by another operator) it must have failed, and the rapidly following death with the post-mortem finding would have suggested the operation as the cause of meningitis; (3) if the right antrum had been operated on at the same time as the left, instead of months previously, it would also have caused suspicion of traumatism or thrombo-phlebitis due to operation as the cause of death.

This case has therefore a medico-legal interest.

Laryngological Section.

December 4, 1914.

Dr. WILLIAM HILL, President of the Section, in the Chair.

Intrinsic Epithelioma of the Larynx One Month after Laryngo-fissure.

By Sir STCLAIR THOMSON, M.D.

THIS gentleman, aged 69, was shown at the last meeting in November. The whole of the left vocal cord was then replaced by a red, knobby, ulcerating infiltration. The cord moved well and there were no enlarged glands.

On November 12, under chloroform anæsthesia, the usual laryngo-fissure was carried out. The growth was found limited to the central three-fifths of the cord, which was clipped out intact in one piece, including the vocal process of the arytænoid. No Hahn's tube was used. Patient swallowed liquids easily the same day and was out of bed within twenty-four hours, eating semi-solid food.

The microscopic examination of the removed left cord showed the structure of an early epithelioma, consisting of a downgrowth of altered squamous epithelium into the underlying tissues. Examination of sections showed that this growth had not penetrated to any distance, and that ample healthy tissue had been removed deeply and anteriorly. But posteriorly the growth had spread right up to the line of excision in the subglottic area—i.e., just below the vocal process of the arytænoid.

As, from the pathologist's point of view, excision could not be looked upon as complete in this region, a second laryngo-fissure was done on November 20, a fortnight ago to-day. The intralaryngeal wound looked well. The bare stump of the arytænoid cartilage showed that the cord had been removed close up to the arytænoid base. A sweep of tissue subglottic to this area was removed and this was

submitted to the pathologist, who reports as follows: "The subglottic tissue was divided into three portions and each piece cut. The middle piece shows a tiny downgrowth of squamous epithelium into the sub-epithelial tissue, which, in view of the known facts of the case, is certainly the posterior edge of the epithelioma. The other sections are free of growth, and hence removal is now doubtless complete."

Patient made as rapid a recovery as after the first operation. He was walking about out of doors a week later. For a week he has been allowed to whisper. There is only a little sinus in the neck leading down to a bare portion of the thyroid cartilage.

DISCUSSION.

Sir StClair Thomson said, in reply to the President (Dr. William Hill), that he would briefly describe this technique, which was only a simplification of the procedure laid down by the late Sir Henry Butlin and Sir Felix Semon. The patient was prepared in the ordinary way and the line of incision was injected with eudrenine, a mixture of adrenalin and eucaine. As a consequence no vessel required tying, and only one vessel had to be clamped throughout the operation. After trying various anæsthetics he had reverted to chloroform. The incision was carried down to expose the thyroid and trachea; then he took an ordinary hypodermic syringe, filled it with 2 per cent. cocaine, and stabbed the trachea before opening. He believed he obtained that idea from an American paper, and Mr. Hope and he had used it for a year or two, both in private and at King's College Hospital. There were several present who saw the operation three weeks ago, and they would agree as to the way in which this intratracheal injection abolished the very inconvenient spasm which used to occur on opening the trachea, causing a great spurt of blood and mucus over the vicinity. One waited a little after the stabbing, and when the tracheotomy tube was inserted there was no reaction. For years past he had abstained from using the Hahn's tube. After splitting the thyroid he put in a tethered sponge from above, and that prevented the blood from getting into the air-passages, and was more reliable than the Hahn's tube. When the growth was exposed three weeks ago it was exactly as shown in the drawing. He clipped it all round. But he wished to confess to an oversight which he must have made. He took the growth out whole, and examined it with his finger to appreciate the cartilaginous consistency, and inspected it with the naked eye; and there seemed to be a clear space all round. Several of those present at the operation agreed that he had gone wide of the growth, and that the result was promising. The specimen was sent to be microscoped, and he asked that three sections should be made. One section showed epithelioma in an early stage; the growth being superficial. The epithelioma was on the surface of the cord, but did not extend into the ventricle of Morgagni, though

it was well down on the subglottic area. Still, removal was free of the growth above, below, and deeply. The second section from the anterior end was also free. A section was also made posteriorly through the vocal process, and the report was that at one part the growth came so close to the edge that it was doubtful if the operation had gone beyond it. In view of this latter finding, he felt that there was no course open but to go back and do his work nearly all over again. Though the patient was aged 70, and had a blood-pressure of 200, he opened up the larynx again in ten days. The cut edge of the vocal process was visible. He took away a piece (indicated) which might have been removed at the first operation. In that corner it did show a thickening of the epithelial cells. He had two cases last summer in which there was recurrence, and in both the disease had extended farther into the subglottic region; and if the growth extended far, it got in between the thyroid and cricoid, and one could not get beyond it. The danger zone extended back not only into the arytænoid but into the subglottic region. He urged the importance of looking into the subglottic region, chiefly posteriorly.

The PRESIDENT said the supplementary details to which they had listened were most valuable. Sir StClair Thomson used the term "laryngo-fissure" as the operation performed, but he (the speaker) imagined it was only fissure of the thyroid cartilage. When he used the term "laryngo-fissure" as synonymous with thyro-fissure, Sir Felix Semon used the term "thyrotomy" or "thyro-fissure" for the minor operation and reserved the term "laryngo-fissure" for vertical median section of the thyroid and the cricoid cartilages. Mr. Tilley had spoken of thyro-chondrotomy as more exact than either thyrotomy or thyro-fissure. Sir StClair Thomson's remark about the danger of leaving some of the disease behind appealed to him, because about eighteen months ago he operated upon a similar case—which was still alive without recurrence—and he found he had to remove the arytænoid cartilage as well to be perfectly certain of getting at least a ¼-in. margin of mucosa free from the disease. It undoubtedly complicated the operation to remove the disease from the anterior surface of the cricoid plate together with the arytænoid. In his case there was trouble in breathing and swallowing through œdema of the aryepiglottic fold left behind, which flopped about like a polypus and had to be snared, and, owing to absence of the arytænoid, for a time food had to be given through a tube to prevent its entering the trachea.

Dr. JOBSON HORNE said that, when he saw the series of three sections of the vocal cord exhibited on the lantern screen, he regretted not having had the opportunity of examining them under the microscope. It was therefore with reservation he raised the question as to the nature of the growth. So far as one could draw conclusions from seeing the sections on the screen, he doubted whether the growth were an epithelioma. There was a redundancy of epithelial tissue along that portion of the cord which is covered with squamous epithelium. He asked whether there was any infiltration of this epithelial overgrowth; that was to say, whether the basement membrane had

been broken through, and whether there was an ingrowth into the substance of the cord itself. When the case was shown prior to the operation, favourable comment was made by more than one speaker, in view of the diagnosis of malignant disease, upon the remarkable free mobility of the vocal cord. That mobility would suggest infiltration had not taken place. The sections exhibited reminded him of some he had met with in studying pachydermia of the larynx. Of course he was not suggesting that the case had presented clinically the typical text-book picture of pachydermia of the larynx; the sections on the screen suggested pachydermia as the pathological condition. He hoped that Sir StClair Thomson would afford them the opportunity of studying the very instructive sections under the microscope.

Dr. DAN MCKENZIE said he was interested in the remark concerning the removal of a portion of the arytænoid, because in a thyrotomy which he had done six or eight weeks ago he found it to be necessary to carry the removal well into the arytænoid region; as a result, swallowing was incommoded and the food came through the tracheotomy tube. The patient developed septic pneumonia, which had proved fatal. There seemed to be borderline cases, in which one was tempted, after having opened the thyroid cartilage, to proceed to remove as much of the disease as could be seen. But in doing so one damaged the swallowing; and in such cases the mortality would necessarily be higher than if the operation were limited to the cases for which the operation was primarily intended. The moral was that when the growth extended beyond the limits proper to classical thyrotomy the larynx should be removed. It was very praiseworthy of Sir StClair Thomson to have opened up the larynx a second time and to have removed the portion of disease left behind. Many in similar circumstances would have been tempted to leave it alone and trust to Providence. He hoped his effort would be crowned with success. When the patient was first shown no specimen, he believed, had been removed for examination; so that the operation was undertaken upon a diagnosis reached without assistance from the pathologist. Probably Sir StClair Thomson would not advise this course in a routine way: and though there might be rare cases in which it was impossible to remove a portion for the pathologist, he (the speaker) would be very chary of operating on any case in which that attempt had not been made. It seemed to him the attempt at least should always be made before proceeding to thyrotomy.

Mr. HERBERT TILLEY said that in 1897 he published a case of ordinary thyro-fissure, in which he removed the arytænoid of set purpose, because the disease extended so far back. The patient got well, and lived for nine years afterwards, then died of recurrence in the other cord. On two or three occasions he had removed half the arytænoid so as not to disturb the anterior part of the lower pharynx. Last year he operated upon a case which Sir Felix Semon had operated upon fifteen years ago; and at the operation a small abscess was discovered with necrosis of the arytænoid cartilage, surrounded by suspicious looking granulations. The patient lived for nine months, but

recurrence of epithelioma took place. Feeding became difficult, and gastrostomy was performed. The patient died with an extensive recurrence over the front of his neck. Probably that had been recorded as a "cured" case, but it showed us that because there was no recurrence of cancer within two or three years of operation it was unwise to speak of "cure." He had not used a Hahn's tube for ten years. He performed a tracheotomy in these cases, packed a long strip of gauze above it—in the trachea—and then split the larynx.

Mr. W. STUART-LOW said he had as a patient a gentleman, aged 69, who appeared to have the same condition. He had been treating him for two years for what he diagnosed as pachydermia laryngis. There was a marked levelling up of the inter-arytænoid space and a thickening and rough-looking appearance of the left vocal cord, but no impairment of movement. What had decided him against this being malignant was, that on palpating the cord with the finger through the mouth—and it was quite possible to do so—there was apparently no induration.

Mr. NORMAN PATTERSON suggested that the microscopic section should be submitted to the Morbid Growths Committee.

Dr. DUNDAS GRANT said it was possible to remove the lower part of the arytænoid cartilage and leave the upper part. He had done that, and found it satisfactory. It was important to preserve the attachment of the sphincter laryngis if possible, for it diminished regurgitation of liquid during drinking, which was, of course, a most serious complication.

Mr. O'MALLEY said the diagram drawn by Sir StClair Thomson showed that the growth had gone beyond the area which was normally covered with squamous epithelium. It reached into the subglottic area, and that seemed to confirm its being an epithelioma.

Sir STCLAIR THOMSON, in reply, said Mr. Gillies saw the specimen, and agreed that it was epithelioma. Still, he would be pleased to submit it to the Morbid Growths Committee.[1] He did not suppose Dr. Horne wished to suggest this as a favourite region for pachydermia laryngis, which had now been clinically defined as a thickening over the vocal process, not in the centre of the cord, as this was. Never having had a death himself, he agreed that this should be an operation free from risk! The risk was swallowing-pneumonia. Of some twenty cases, he had had to feed only one through the nose, for a week or two. Removing part of the arytænoid complicated the conditions. On looking into the larynx a week later, one saw a horrible œdematous condition of the remains of the arytænoid, and frequently of the other arytænoid. In one case he had to enter again, by the direct method, and get the shifting mass out. He agreed with Dr. McKenzie as to the desirability, if possible, of removing a portion

[1] The Committee have since reported on the growth as being a typical epithelioma.

beforehand for examination. But in this case he did not think that was feasible; and if the report had been negative he would still have gone on with the operation. Therefore, of what use would a preliminary examination have been? As he had learned from Dr. Bond, of Golden Square, he was accustomed to feel with his finger round the epiglottis and introitus of the larynx, but he had never got as far as the glottis in adults.

Demonstration of Foreign Bodies removed from Air Passages.

By Sir W. MILLIGAN, M.D.

(1) *Damson-stone removed from Left Bronchus.* — A boy, aged 4, while eating damson jam accidentally inhaled a stone. Sharp attack of dyspnœa. During following ten days several attacks of dyspnœa accompanied by spasmodic attacks of coughing. X-ray examination negative. On admission to Royal Infirmary the child was found to be suffering from slight bronchitis with diminished entry of air into left lung. Examination under general anæsthesia. Sudden arrest of respiration; performance of rapid tracheotomy and artificial respiration, followed by recovery. Child put back to bed for half an hour; again anæsthetised; tracheotomy tube withdrawn and bronchoscope tube passed. Damson-stone seen in left bronchus with pointed end protruding (specimen shown). Successfully recovered with forceps. Rapid recovery.

(2) *Carpet-tack removed from Bronchus of a Female.* — Female, aged 35, accidentally inhaled a carpet-tack which she was holding in her mouth when engaged tacking down a carpet. Sudden and severe paroxysm of dyspnœa. During the following four days several slight attacks of dyspnœa and coughing fits. Admitted to Royal Infirmary eight days after accident. (X-ray photograph shown of the tack in situ.) Under general anæsthesia tack grasped, but on traction being made forceps slipped off. Many unsuccessful attempts at extraction made. Patient kept quietly in bed for three days. Vapor benzoin co. administered every two hours. Patient again anæsthetised and removed to X-ray dark room. Fluorescent screen employed and forceps made to grasp tack. Withdrawal again unsuccessful owing to forceps not holding the stem of the tack sufficiently firmly. Many more unsuccessful attempts at extraction. At the suggestion of Mr. Barclay

(Radiographer to the Royal Infirmary) an attempt was made to twist the tack round upon its own axis. After much trouble this was accomplished and the tack was immediately withdrawn by grasping its head (specimen shown). Despite prolonged manipulations on these two occasions no systemic disturbance followed and the patient left hospital upon the third day in perfect health.

(3) *Halfpenny in Œsophagus of a Child, aged* 2.—Coin readily seen impacted at about the level of the sterno-clavicular joint. In situ for ten days. Removal with forceps. Œsophageal wall intact. Rapid recovery (specimen shown).

These cases demonstrated: (1) the tolerance of the tissues under certain conditions; (2) the advisability, if not necessity, of not relying upon the results of radiography; (3) the value of the fluorescent screen as an aid to the removal of foreign bodies from the lung.

DISCUSSION.

The PRESIDENT recommended the method which he had first seen Mr. Tilley use, for removing a foreign body from the bronchus—viz., having the patient on the X-ray table, and guiding the forceps by means of the screen. He had himself succeeded on one occasion in which he had previously failed to get the foreign body by the ordinary procedure.

Mr. TILLEY congratulated the radiographer on his beautiful skiagram. He suggested that in the case of a foreign body in the lung, where good definition was not achieved, instantaneous radiography would then secure good definition. That was pointed out to him by Dr. Finzi. He agreed that one must not absolutely rely on the results of radiography.

Sir STCLAIR THOMSON said one was often successful when trying to remove the whole apparatus *en masse*, because it had happened to him more than once that the head of a shawl-pin or a reversed tack had caught against the end of forceps, and the forceps came out minus the foreign body. And in the case of larger things, such as tooth-plates, one could pretty safely remove the whole thing together.

Dr. D. R. PATERSON said he had a case similar to the second case, and as he could not get enough control over the tack with a long forceps he did a low tracheotomy. A shorter forceps enabled him to manipulate and extract very easily. The child was about 3 years of age.

Dr. IRWIN MOORE asked Sir William Milligan whose forceps, or which type of forceps, he had used in this case. If Brünings's forceps were used

which end had been tried? He ventured to say that if Sir William Milligan had used the forceps which he (Dr. Moore) had introduced two years ago for use with Brünings's tubes for the removal of foreign bodies, the difficulties met with in this case would not have been experienced. The forceps introduced by Brünings as shown by this case would grasp a tack by the stem, but the grip would not hold—this was the weak point of the forceps in the removal of foreign bodies. Whereas the forceps which he (Dr. Moore) had introduced would easily grip a tack by the point, firmly hold it, and successfully remove it. (The way in which the forceps seized the point of a carpet tack and the secure hold it had of it was demonstrated.) He had shown this forceps at the British Medical Association Meeting at Liverpool in 1912, and since then it had been used by many of his colleagues. In fact, he had not known of a failure when this forceps had been used.

The PRESIDENT said that Brünings's and Killian's forceps were very fragile, and cases had been recorded in which instead of one foreign body in the patient there were three—the original foreign body and two portions of forceps. Dr. Irwin Moore's œsophageal forceps was a very good instrument.

Mr. O'MALLEY said he had had some similar experiences when trying to extract foreign bodies with Brünings's forceps; the pressure of the spring of the forceps tended to withdraw the prehensile portion into the casing, and it was difficult to be sure whether a small object was really being grasped. For this reason he considered that Dr. Irwin Moore's forceps was a decided improvement on Brünings's.

Mr. E. D. D. DAVIS said he had been collecting records of cases similar to the third one. In this case there was neither the history of the swallowing of a foreign body nor the indication for a skiagram, and a diagnosis was well-nigh impossible; but in some cases there was a history of the swallowing or inhalation of an object, but enough attention had not been paid to it. For example, in one case, a swallowed halfpenny lodged in almost the same position, and ulcerated into the aorta, and the child, aged 7, died of hæmatemesis. The history was that about five years after the swallowing of the coin he had vomited a little blood, and in the night he had a profuse hæmorrhage and died. In another case an ordinary pin had passed through the œsophagus into the aorta, and the patient died from hæmorrhage. Dr. Rolleston sent a case to him at Charing Cross Hospital, with the history that four months ago a rabbit bone had been swallowed. At the casualty department of another hospital not much attention was paid to the history, and the child was sent away. It returned a week or two later, and was admitted and treated for bronchitis for three weeks, and then discharged; a week later it was sent to the Fever Hospital as a case of diphtheria. Dr. Rolleston, suspecting a foreign body, sent the case to him (the speaker). He examined the child with the laryngoscope and a greyish object was seen, which he felt confident was the lost rabbit bone. The bone was impacted

edge-on between the vocal cords, so that there was room for air to pass on either side. Had it fallen flat, the child would probably have been suffocated. The bone was removed by Killian's suspension laryngoscopy under chloroform. In a case of his own, a safety-pin was removed by the same method; the pin was open and sticking into the wall of the pharynx opposite the cricoid cartilage. His colleague, Mr. Waggett, and he had been in the habit, when a patient was alleged to have swallowed or inhaled a foreign body, of making an ordinary examination of the pharynx and larynx; sometimes a sharp foreign body lodged in the tonsil or the base of the tongue. If that failed, a skiagram was taken, the stools watched, and finally they resorted to the œsophagoscope or bronchoscope, but in every case the foreign body should be accounted for. Many foreign bodies had been missed because the history was difficult to obtain or the child had very few and slight symptoms, or symptoms simulating other affections. This child was difficult to examine, she had glands in the neck, aphonia, stridor, &c., and he thought she had enlarged mediastinal glands. At a second examination with the laryngoscope a few minutes later he could see the bone in the larynx. Suspension laryngoscopy was therefore performed under chloroform. He did not attempt to feel the bone with the finger, because of the danger of dislodging the bone, and producing severe dyspnœa.

Sir WILLIAM MILLIGAN, in reply, said he had been in the habit of carrying out removal *en masse*. The point raised by Dr. Paterson, as to more frequently doing a low tracheotomy in these cases, was an important one. He had gone on the rule that in young children it was advisable to do a low tracheotomy; but not in adults. He had nothing but praise for Dr. Irwin Moore's forceps. Dr. Moore once lent him a pair of the forceps when he was in a very great difficulty, and it had the best grasp of any forceps he had ever handled. That of Brünings was really a toy when it came to anything serious. Had it not been for the help of the radiologist, the foreign body would have been there still. He agreed that one should not rely entirely on the radiogram. He had had the radiographer's reports that there was no foreign body present, yet he had found them with the bronchoscope. In one case a child swallowed a bean, and in ten days it had marked respiratory dyspnœa. But the radiogram was negative. He found the bean lying on the carina. During the ten days some portion of the bean must have got detached, for septic pneumonia followed by a lung abscess developed, and the child died.

Case of Inoperable Angio-fibroma; Maxillary Antral Sarcoma (?). (Microscopic Section.)

By Sir W. Milligan, M.D.

Patient, a male, aged 17. Three years' history of unilateral nasal growth. Two attempts at removal had been made before he came under observation. Patient complained of left-sided nasal obstruction and intermittent attacks of hæmorrhage and pain. The left nasal passage was completely blocked by a polypoid growth with smooth, glistening surface traversed by somewhat enlarged blood-vessels. On posterior rhinoscopy the growth was seen to occupy a considerable portion of the nasopharynx. The soft palate was pushed forwards. The left side of the face appeared much swollen, due to distension of the bony walls of the antrum. The antrum was quite opaque upon transillumination.

Operation: Kuhn's per-oral intubation tube was passed and the mouth was packed with gauze. Lateral rhinotomy was performed. On attempting to remove the growth there was furious hæmorrhage. The cavity was rapidly packed. Several attempts were made to remove the growth, but unsuccessfully on account of the extraordinarily severe bleeding. Patient collapsed and the operation was stopped. The cavity was tightly packed with gauze and saline administered. Forty-eight hours afterwards the packing was removed; again there was very severe hæmorrhage. Radium emanation tube (50 mgrm.) was inserted into the middle of the growth. No return of hæmorrhage. Rapid retrogression of growth. Three weeks afterwards there was no trace of growth, but a dense, firm, fibrous mass was found attached to the posterior antral wall (microscopic section shown). The general and local condition of the patient was now excellent.

DISCUSSION.

The President noted that the case was a hæmorrhagic one, and that radium had acted as a hæmostatic. He had usually embedded from underneath the lip, and he had no serious hæmorrhage. In one case of sarcoma of the anterior wall of the antrum it disappeared completely under radium in two days without embedding.

Dr. Jobson Horne inquired about the site of origin of the growth; whether it arose in the basi-sphenoid and spread to the antrum, or within the antrum

and extended to the adjacent parts. That point had an important bearing on the treatment. To get rid of such growths it was necessary to get behind them and to attack the seat of origin. Dr. Jobson Horne preferred the title angio-fibromatosis before the second title, suggested by the exhibitor, of maxillary antral sarcoma. Some years ago he had given reasons for not regarding these growths as sarcomata. They destroyed by local extension and not by metastases. The growths consisting mainly of embryonic tissue rapidly retrogressed under radium.

Mr. TILLEY said he had had to deal with a case in which a large growth seemed hopeless in nature and situation, but after putting in radium it had disappeared completely; there was now no disfigurement. Ten days after putting in the radium he passed an exploring needle into the antrum and washed out a quantity of very foul smelling material. He asked why Sir William Milligan preferred to operate before using the radium.

The PRESIDENT said many believed in applying radium in the first instance in cases of sarcoma. He had a case of growth the size of a turnip at the hospital, and 200 mgrm. of radium were being applied at the moment. It was a mixture of round and spindle cells, which did not yield so well to radium as one of purely round cells.

Mr. HUNTER TOD asked whether, in these cases, the exhibitor had tied the external carotid. He had done so recently in a similar case, with the result that there was practically no hæmorrhage.

Dr. DUNDAS GRANT thought it an encouraging illustration of the use of radium, and asked how long it was since the operation was done; he presumed long enough to test whether there was likely to be recurrence. Recently, with the aid of Dr. Knox, he introduced a tube of radium into the nasopharynx in a case of round-celled sarcoma, and caused absolute stoppage of the nose in consequence; there followed a most striking yielding of the growth to the radium and restoration of nasal patency. The patient did not come back, as agreed, a few weeks later to have it repeated, and he died. With Sir William Milligan's experience perhaps most members would try radium in the first place in such a case, as perhaps Sir William himself would. A firm fibrous mass was described. Was that information derived by inspection or palpation?

Sir WILLIAM MILLIGAN replied that he doubted if the growth was sarcoma; he regarded it as a vascular angiofibroma. But the clinical aspects of the case made one think there must be something more at the bottom of it, because the growth had been twice removed before the patient came to the infirmary. Had he felt certain it was sarcoma he would probably have used the radium in the first instance, but he operated in the belief that it was a large vascular innocent growth. It grew from the posterior wall of the antrum, and extended back into the nasopharynx; it was not in the vault of the pharynx; there was a

small space in which it did not touch the vault or the roof. He had tied the external carotid in a large number of similar cases, but he did not do it in this case; he could not say why, although when he did it in other cases it had proved of great advantage. His query in the title was due to the clinical course of the case. The operation was done three months ago. One of the immediate effects of the radium was to arrest the hæmorrhage, which at the time of the operation was furious. Though only 50 mgrm. were used the effect was wonderful. The remains of the growth could be seen from the front; there was practically no lateral wall of the antrum, it had been absorbed as the growth extended. There was need for a revision of ideas as to the degree of malignancy of these growths. Some which the microscope led one to think were very malignant turned out to be clinically quite mild.

A Naso-antral or Choanal Polypus, upon the Posterior Surface of which is an Ulcer the Size of a Shilling.

By Herbert Tilley, F.R.C.S.

Mr. Tilley said that when he looked at the polypus with the posterior mirror the mass looked like a tertiary syphilitic ulcer. He thought that what he saw by anterior rhinoscopy was œdema probably connected with the ulcer; the latter had a red, well-marked border, with sloughy base. On removal he found an ulcer on the polypus; he had never seen anything quite similar. He did not open the antrum, because out of many cases of choanal polypus he had only had to make a second removal in three of them. He evulsed them the first time with forceps, and if there was more than one recurrence he opened the antrum through the canine fossa.

DISCUSSION.

Mr. Clayton Fox asked whether the polypus was cystic. A few years ago he had to deal with a very decided case of this nature, in which he did two removals, and on each occasion 2 or 3 dr. of clear serous fluid escaped from the pedicle after removal. By means of a small mirror he could see through the accessory opening in the naso-antral wall.

Dr. Dan McKenzie said, that sometimes antro-nasal polypi became gangrenous from kinking round the accessory opening. Possibly that accounted for the ulceration in this case. He agreed that single removal sufficed for most cases. German authorities contended that it was necessary to open the antrum in all such cases; but he dissented from that.

Dr. DUNDAS GRANT said it was desirable to make sure, by palpation, whether there was a large opening. Forceps could be guided into the antrum and one could tear and scrape away with the assurance that recurrence would not take place. But he had seen cases in which recurrence took place after a number of years.

Sir WILLIAM MILLIGAN raised the question as to the possibility of this ulceration having been self-inflicted by the use of the finger or the tongue. He had seen patients able to put their tongue to the very back of the nasopharynx.

Mr. CLAYTON FOX suggested that the ulceration might be due to simple friction and pressure.

The PRESIDENT said these cases did not often recur. Mr. Harmer had been injecting bismuth into antral cavities. The amount of bismuth used would give an indication of the size of the cavity; and if the antrum were filled with polypus, its outline would be different from that seen in the normal antrum by X-ray examination. Whether it was worth while to fill up the antrum to make a diagnosis was a debatable matter.

Mr. TILLEY replied that the possible factitious origin of the ulceration had not occurred to him; but he did not think it was probable, because this patient had a long, narrow, high-arched palate, and it would have been impossible for him to insert his tongue into his nasopharynx. He believed there was always a cyst in these naso-antral polypi.

Retropharyngeal Swelling.

By G. J. F. ELPHICK.

PATIENT noticed a "swelling in the throat" about the end of September, 1914, which caused slight difficulty in swallowing. There is a rounded swelling visible on the posterior pharyngeal wall towards the left, which has a definite right border, is hard to the touch, and not very tender. Patient has occasional pain radiating up the left side of the neck towards the head, and is certain that the swelling is getting larger. There are no enlarged glands in the neck. There is no limitation of cervical movements, and a skiagram excludes cervical caries. The Wassermann reaction is being tested. Opinions are invited as to the diagnosis and treatment of this case.

The PRESIDENT said the exhibitor had left, but the Wassermann test was positive, and it was evidently a gumma. There was an obvious large swelling when first seen, and it was thought to be possibly a parotid tumour.

Tuberculoma of Nasal Septum.

By C. W. M. Hope, F.R.C.S.

E. L., aged 24, single. History of two and half-years' nasal obstruction right side, with very slight bleeding, and soreness at anterior nares. No pain.

On admission to King's College Hospital: Pedunculated cauliflower-like growth from septum blocking up right anterior nares. Small patch of lupus scarring in post-nasal space; portion removed for microscopic examination from septal growth. Report from Dr. Emery: "Tuberculoma." Chest examination: No lung affection.

Operation, November 11, 1914, under cocaine anæsthesia. Whole of right septal mucous membrane for a distance of 2 in. from vestibule and almost up to roof found affected; removed by stripping away underlying perichondrium. Cartilage and bone not touched. Floor of nostril and inferior meatus found affected and treated in the same way. Anterior two-thirds of superior turbinal also found affected and removed.

Sections and mass from septum are shown.

DISCUSSION.

Dr. Vinrace asked Sir StClair Thomson, as Mr. Hope's deputy in his absence, whether there was evidence of tuberculosis or lupus in any other part of the body, or whether such existed at the present time. In effect, the diagnosis was primary tuberculosis of local nature in the nasopharyngeal space: and if that were correct one would have expected there would have been manifestations of the disease elsewhere. He would call it a chronic inflammatory growth.

Dr. Jobson Horne said the section exhibited under the microscope appeared to be stained for tubercle bacilli, but it was impossible to say whether they were present in the section, as it was not under an oil immersion. He had microscoped growths from similar cases of so-called tuberculoma of the nose, arising generally from the septum; he usually found a definite giant cell formation, but he could not find tubercle bacilli, and guinea-pigs inoculated with the tissue did not respond. It would be a pity to regard such cases as instances of primary tuberculosis of the nose.

Mr. Howarth said that giant cell systems were commonly seen in all granulomata. Clinically the condition was lupus.

Dr. DUNDAS GRANT said the difficulty of finding tubercle bacilli in lupus was well known. This case accorded with the typical description of that mitigated form of tuberculosis of the nasal septum, and it seemed a likely spot for infection. It was seen in people who had no other sign of tuberculosis.

Sir WILLIAM MILLIGAN doubted whether this was primary tuberculosis, from the clinical point of view. He regarded the case as lupus with proliferating granulomatous changes, the result of chronicity. He had never seen tubercle bacilli in a lupus section. He regarded lupus as a non-bacillary form of tuberculosis.

Mr. CLAYTON FOX asked what was the disposition of the giant cells in the pathological specimen. Were the giant cells in the periphery? If so, it was in favour of lupus, which was modified tuberculosis.

Sir STCLAIR THOMSON, replying for Mr. Hope, said he had this case at heart, because he had watched a similar one for seventeen years. The present case was sent to his clinic as a suspected case of malignant disease, because of the fungating sessile growth on the right side of the septum. It had existed for some time, and the youth of the woman did not contraindicate its malignancy. But as it had not penetrated the septum, and there were no enlarged glands, it was clinically non-malignant. He agreed that histologically it was a tuberculoma, and clinically it was lupus. He raised this question at the old Laryngological Society of London and it involved him in a discussion with Professor Massei, of Naples, who was so pathological that he wanted to abolish lupus, and class what was known as lupus as a mitigated form of tuberculosis. He (the speaker), however, held that it was important to distinguish between them. This was a sessile growth, going farther back than was expected, and on the posterior wall of the nasopharynx there was a glazed and infiltrated condition which he regarded as the next step in the lupus infection. He gave a diagram of what happened in a similar condition seventeen years ago. It was a case of tuberculoma shown by Dr. Watson Williams at the old Society, and he had watched the growth penetrate into the other side of the nose, and infect the inferior turbinal and go into the nasopharynx. The patient married, and he warned her against having children. But she did have children, and each pregnancy led to further extension of the disease. It invaded her larynx, and she developed it on her face; and she now had tuberculosis of the lung, and was probably dying. The giant cell system was the only pathological evidence that this was early lupus in the form of a tuberculoma. Tubercle bacilli were rarely discoverable in the tuberculomata or lupus growths, although, of course, Koch's bacillus was the primary cause.

An Unusual Case of Enlarged Tonsils.

By C. W. M. Hope, F.R.C.S.

A. F., aged 29, came to Golden Square Hospital on November 14, 1914, to have his tonsils removed. There was no complaint except that the tonsils had troubled him for the past few weeks.

On examination: Tonsils size of large walnuts, rather hard, surface covered with irregular, dark nævoid patches; masses of soft, movable glands, both sides of neck and in each axilla; bluish nodules in skin over chest and abdomen, up to the size of a shilling-piece; marked anæmia.

Diagnosis: ? Sarcoma of tonsils, with sarcomatosis cutis.

Gland from neck and a nodule from skin of chest removed for microscopic examination. Report from Mr. Gillies: "Lymphatic leukæmia." Sections shown.

Blood count not made, as the patient has not been up to the hospital since his first visit.

Two Cases of Injury to the Nose with subsequent Deformity.

By W. Stuart-Low, F.R.C.S.

Case I.—A boy, aged 4, who five weeks ago fell in the road and struck his nose. The nose bled considerably, but there was no abrasion of the skin. There is now a firm thickening on the left side over the alar cartilage and the frontal process of maxilla; the lower edge of the alar cartilage is retracted and the deformity is increasing. There has evidently been a periostitis and perichondritis, and some organisation of the extravasated blood. The swelling is unusually firm and is gradually increasing, and the lumen of the nostril is being narrowed. Suggestions as regards treatment are requested.

Case II.—A girl, aged 9, who fell in the road five weeks ago, striking her forehead on a piece of iron. Rapid effusion of blood took place, but there was no abrasion. There is now present a hard mass above the left inner canthus, causing marked deformity. The friends say it is slowly enlarging. Excision is proposed.

In neither of these cases was there hæmatoma of the nasal septum, but in the case of the girl there was probably effusion of blood in the left ethmoidal region, as the nose bled freely and was obstructed for days.

Tubes of Soft Metal for Insertion into Nose after Submucous Resection.

By DAN MCKENZIE, M.D.

THE tubes are inserted immediately after the operation and the packing applied round them. Their use does away with the objectionable blockage of the nose inseparable otherwise from packing after resection.

DISCUSSION.

Mr. HARMER said that he had used rubber tubes in the nose after submucous resection for four years. When placed in the inferior meatus they kept the septum in position and very little plugging was necessary. In most instances they enabled the patient to breathe comfortably through the nose. The use of tubes was originated at St. Bartholomew's Hospital by Mr. Walsham, who claimed that in addition to making the patient comfortable they acted as drains and prevented the formation of adhesions.

Dr. JOBSON HORNE said he was glad these tubes were being re-introduced. Mr. Walsham, he remembered, used ordinary rubber drainage-tubes passed through the nose after septum operations. The tubes were perforated. The perforating was an advantage.

Sir WILLIAM MILLIGAN agreed that they were excellent tubes, but it raised the question whether tubes were desirable at all.

Mr. W. STUART-LOW thought this question would make an excellent subject for a set discussion.

Dr. MCKENZIE replied that the tubes were not intended for drainage; they were for the patient to breathe through during the time the packing was in the nose. Pressure of the swelling caused rubber tubes to close. He thought anyone who had once tried his tubes for twenty-four hours after the operation would continue to use them.

Case of New Growth of Tonsil and Faucial Pillars, with Microscopic Specimen.

By H. Clayton Fox, F.R.C.S.I.

The exhibitor thought the growth was an epithelioma; the pathologist, on the contrary, pronounced it to be an infective papilloma.

DISCUSSION.

Mr. Stuart-Low thought it was epithelioma. There was induration, and on palpation it was very firm. In a similar case, radium administration had done good.

Dr. Vinrace asked whether the Wassermann test had been applied, and why there was no glandular enlargement. The faucial pillar was eroded and infiltrated and had an angry appearance, and it was as likely to be syphilis as malignant disease.

Dr. Leathem said he had examined the specimen, and regarded it as infective papilloma. The most doubtful part of the specimen showed one or two epithelial masses resembling cell-nests, but on the whole he thought it was papilloma.

Dr. Dundas Grant regarded it as epithelioma: it was diffuse and ulcerated, and there were cell-nests. If so it was a most instructive case, showing how latent a malignant process might be.

Mr. Clayton Fox, in reply, said that Escat in his work on diseases of the throat mentioned the fact that cervical abscesses had been observed as the earliest manifestation of latent epithelioma of the pharynx. He believed that in this case the epitheliomatous growth had started in the supra-tonsillar fossa or in a palatine recess of the same. Wassermann's reaction had not been tested. He wondered whether the early overrunning of the area involved, together with the subsequent cicatrisation, might not have had some influence in blocking the lymphatics, and thus accounting for the absence of glandular enlargement met with in this case.

Laryngological Section.

February 5, 1915.

Dr. William Hill, President of the Section, in the Chair.

Announcement.

The President (Dr. William Hill) said the Council had been considering the question of holding a special discussion, and it had been decided to devote the afternoon meeting in May to a symposium on warfare wounds, injuries, and neuroses in the air passages and upper food passages, excluding ear troubles. He invited members to send notification of their intention of showing patients and specimens or records of cases or even short papers, so that the agenda might be tabulated and arranged and circulated well in advance. In answer to a question by Mr. Tilley, he said he did not see why medical, and even functional, conditions resulting from warfare should not be included, provided the nose, throat, gullet, trachea, &c., were affected.

A Note on the After-treatment of Cases of Submucous Resection of the Nasal Septum without the Use of Splints.

By Sir William Milligan, M.D.

Sir William Milligan said that at the last meeting of the Section Dr. Dan McKenzie showed new splints for the after-treatment of cases in which a submucous resection of the nasal septum had been performed, but he considered that if these cases could be treated without a splint at all it would be a considerable advantage. The comfort to the patient was undoubtedly greater, and continuous drainage from the nose was better ensured. For some time he had not used any splints. After the requisite bone and cartilage had been removed he used Whitehead's

varnish,[1] a varnish used by Mr. Walter Whitehead after his operation for removing the tongue, painting it on the two perichondrial surfaces, and then bringing those surfaces together. It caused the muco-perichondrial surfaces to adhere. Though he had used this method for a considerable time, he had not seen a hæmatoma ensue in any case, and patients expressed their appreciation of being able to breathe through the nose immediately after the operation. The opinion of the nurses was that recovery was more rapid under this plan than when splints were employed.

DISCUSSION.

Dr. WATSON WILLIAMS said that when, as often happened, there were complicated conditions present, e.g., more or less active sinus trouble, packing of the nose was a cause of considerable anxiety; sometimes one did not know whether to deal with the septum or the sinus first. Again, the sinus might be difficult to pack and operate on through the nose when the septum was much deflected. Hence dispensing with packing became a most useful procedure.

Dr. DUNDAS GRANT said this varnish seemed to be the one thing wanted for the prevention of the formation of hæmatoma without plugging. He had in some cases passed stitches from one side to the other, and this had acted very well. Another plan which answered well was to make a perforation far back on the side opposite to that of the original incision, to allow of the escape of blood. It was in the cases in which the technique seemed most successful that hæmatoma seemed most likely to form. He remembered having tried some ingenious clamps devised by Brünings, but his own patients could not tolerate them and they were given up.

Dr. DAN MCKENZIE asked whether, in the cases in which an artery was cut low down in the nasal crest, the varnish sufficed to control the hæmorrhage. The packing was used to prevent a hæmatoma after the effect of the adrenalin had worn off.

Sir STCLAIR THOMSON said that dispensing with plugging was suggested in America some eighteen months ago, and urged in the *Laryngoscope* and other journals. He had never put in plugs when there were sinus complications, and the septum did very well. For some five years now his practice had been to do a resection of the septum, and then put the septum practically entirely back again, removing only any redundant pieces. He made a kind of mosaic of the portions and put them back, packing them between the two flaps. To keep them in position for the first twenty-four hours he put in small rubber sponge plugs, and there was seldom any complaint.

[1] Gum benzoin, 4 dr.; styrax, 3 dr.; socotrine aloes, 40 gr.; balsam of Tolu, 1 dr.; iodoform, 4 dr.; pure ether, 5 oz.

Mr. HERBERT TILLEY said he had not tried the varnish in such cases, but it was very useful as a styptic antiseptic in operations on the larynx for the removal of malignant disease, in order to stop bleeding in the deep posterior part of the removed cord. The evil of the splint was the discomfort to the patient during the first twenty-four hours. For the last six months he had been using an oval drainage-tube, through which the patient could breathe. It was anchored in position by a piece of plaster on the cheek.

Mr. HOPE said he had never used an ordinary hard splint for these cases, but employed iodoform gauze soaked in vaseline, and he did not find patients complained of it. He had also used a small piece of rubber tubing on the floor of the nose, and packing on the top of it; but some patients complained of that.

Sir WILLIAM MILLIGAN replied that patients complained of discomfort even when he used the simplest of splints—i.e., a rubber finger-stall lightly filled with gauze and smeared over with vaseline. In answer to Dr. McKenzie, if the naso-palatine artery bled freely, it was best to needle it with a Krause's hook, and then on painting it over with the varnish the bleeding stopped. He was not aware the method had been used in America; he did not believe it had been employed in this country. He had not performed the operation described by Sir StClair Thomson; when he took the cartilage away he was glad to get rid of it. He found that an excellent septum formed from the fibro-cartilaginous structures.

Photographs of a Case of Chronic Lymphangitis of the Tissues covering the Nose; Hints as to Treatment desired.

By Sir WILLIAM MILLIGAN, M.D.

THE patient was a woman, aged 30, of healthy history, but at the age of 18 she had a very bad nasal catarrh, which disappeared after lasting some months. Three years ago she began to complain of her nose becoming enlarged. The interior of the nose did not seem to depart much from the normal, nor did the pharynx or nasopharynx. He suggested it was a chronic lymphangitis. If any reasonable treatment could be suggested he would take her into hospital and carry it out.

DISCUSSION.

Dr. WATSON WILLIAMS asked whether sinus affection had been excluded. In the last edition of his text-book he illustrated two cases of somewhat similar lymphangitis of the nose, in one of which the lip was involved; and in

both he found latent sinus trouble. There might not be discharge or gross evidence of that, but careful washing out of the antra and culturing the discharge might give a clue. He did not think the sphenoidal sinus was likely to be at fault. The chronic condition was more likely to cause trouble than the more acute and manifest conditions, in which many polymorphs were poured out.

Mr. STUART-LOW said that some years ago he had a similar case (a young lady) under treatment. There was considerable improvement in the external appearance of the nose after repeated scarification of the interior of the nasal passages; this caused very free bleeding at the time and resulted in great diminution of the swelling and of the redness of the nose.

Dr. SYME did not think this condition was chronic œdema such as was met with in disease of the sinuses. From the photograph he would judge it was a case of simple lymphangitis. He had seen the auricle in the same condition. More harm than good was likely to follow interference.

Dr. DUNDAS GRANT asked whether anything in the way of elastic compression could be devised. Such compression had been found of benefit in elephantiasis or lymphangiectasis of the leg when Martin's india-rubber bandages came into vogue.

Mr. HERBERT TILLEY suggested that before attempting any operation an attempt should be made to obtain a culture from the blood; that might give an indication as to the possible source of the trouble, and an autogenous vaccine might be made. After the South African War he had a case involving eyelids and forehead; the sinuses were opened by another surgeon and nothing abnormal found. Eventually, an anti-streptococcic serum did most good, an attenuated form of that micro-organism having been discovered in the exudations of the swollen areas.

Dr. BROECKAERT (speaking in French) said it would be well, in this case, to take out a wedge and make use of flaps. He considered that the scars would not be very disfiguring afterwards.

Sir WILLIAM MILLIGAN replied that he transilluminated the patient, but did not make a puncture. He would have a skiagram of the frontal sinuses taken. He did not see how internal scarification was likely to benefit this case, but he would bear the suggestion in mind. He had thought of taking a culture, but had not yet done so; but he proposed to adopt the suggestion at once. He did not think the case sufficiently severe to justify the carrying out of Dr. Broeckaert's suggestion. The doctor who sent the case said that diathermy had been tried. He was grateful for the various hints, and he was inclined to try Mr. Tilley's suggestion first.

A Nasopharyngeal Fibroma enucleated by a Curved Dissector and the Finger.

By E. D. Davis, F.R.C.S.

F. W., a boy, aged 14, was sent to hospital on July 20, 1914, for the removal of an adenoid growth. Six years before, a tonsil and adenoid operation had been performed at a throat hospital. The patient was suffering from nasal obstruction, with slight deafness, but there was neither epistaxis nor pain. The post-nasal mirror showed a large globular smooth swelling filling the nasopharynx and attached to the body of the sphenoid. The tumour could be seen from the right anterior nares, and almost completely obstructed the corresponding choana. The tumour was elastic, firmly attached, and had caused absorption of the posterior edge of the vomer and hard palate. He was examined under an anæsthetic, and the growth was considered to be a sarcoma. A hundred milligramme radium emanation tube was inserted into the tumour through the right anterior nares with no result. A section of a piece of the growth was found to be a fibroma.

In August, 1914, the tumour was completely enucleated with a strong dissector and the finger without splitting the palate. (The bifid uvula is congenital.) The operation was considerably facilitated by the administration of intratracheal ether by Dr. G. Ramsay Phillips.

The patient, who can be examined by the mirror and the endoscope, is shown. Also the tumour (2½ in. by 1½ in. in size) and histological specimens.

DISCUSSION.

Mr. Herbert Tilley asked whether "enucleated" was the right term to use. If the tumour was enucleated, he would like to hear what had been done with the capsule. He believed these growths gradually grew from a broad fibrous base.

Dr. Fitzgerald Powell supported Mr. Tilley's remarks. Considerable force was required to remove these fibromata with a raspatory, as they were usually firmly attached to the base of the skull and extensive in their attachment. Of course, such methods were only suitable for small growths, and would be quite useless in the larger fibromata which were fixed to the basi-sphenoid, pterygoid plate, &c.

Dr. DUNDAS GRANT said that for these cases he had used a periosteal elevator, passing it down through the nose, and guiding it with his finger to the under surface of the sphenoid, and possibly to the adjoining pterygoid plate. He used considerable force, so as to detach the periosteum with the tumour. In his cases the attachments had been very widespread. The cases where the growth arose from the antrum should, of course, be differentiated from these. He believed these fibromata grew from the periosteum, and he asked whether Mr. Davis passed the elevator through the nose or behind the soft palate. Doyen devised an instrument for working behind the soft palate, but he (the speaker) had found it better to work through the nose.

Dr. D. R. PATERSON said he had had several cases of the kind, which he had operated upon in the way Dr. Dundas Grant had just described. The growth was seized by a vulsellum from below and traction made by an assistant. A long, narrow periosteum elevator was then passed, first through one nostril, then through the other, towards the nasopharynx, one or two fingers of the left hand being pushed behind the soft palate to the same spot. The seat of the attachment was in this way dealt with and the growth detached and pulled away. There was very little hæmorrhage, and the result in each case was most satisfactory.

Sir WILLIAM MILLIGAN said the case recalled to his mind that of a boy upon whom he had operated fifteen years ago by means of a long carpenter's chisel ½ in. broad, using it as an elevator passed through the nose. The growth was levered out; there was no capsule. Using the finger in the nasopharynx as a guide, he regarded it as an excellent method, and he had done it several times since.

The PRESIDENT said the method employed must depend upon whether the growth was loose or was firmly attached over a wide area.

Mr. E. D. DAVIS replied that he first struggled with the growth by attacking it through the nose with snares and a small dissector. Then he tried lifting the soft palate with a retractor, which gave him a good view of the tumour. He used Doyen's raspatory, and after reflecting the mucous membrane, to his pleasant surprise he found the right layer and the tumour peeled out quite easily with the finger. He used the term "enucleated" metaphorically and only in the sense in which it was applied to the removal of a prostate. He also attempted to remove it with Sir StClair Thomson's adenoid forceps and then with a huge nasopharyngeal forceps, but could not make any impression on it. Intratracheal ether was a most useful anæsthetic and greatly facilitated the operation. At the conclusion of the operation the bleeding was so slight that he did not plug the nasopharynx, but this had to be done four hours later.

A Post-mortem Specimen of a Pituitary Cyst opened by the Killian-Hirsch Operation.

By E. D. DAVIS, F.R.C.S.

A MAN, aged 37, was sent for operation to relieve progressive blindness. In April, 1914, the patient was seen by Mr. Treacher Collins, who detected optic atrophy and signs of hypopituitarism. A skiagram revealed a large sella turcica with absorption of the dorsum sellæ.

July 26: The Killian-Hirsch operation was satisfactorily performed, but the hæmorrhage on incision of the dura was profuse. On the next day hemiplegia developed, with loss of speech, paralysis of the right face, right arm and leg.

July 30: The sight improved and the patient could read a newspaper.

August 12: Severe occipital headache and restlessness occurred. Temperature 103° F. Two days later the temperature was normal and remained about the normal for nine days and then a similar attack of pain, restlessness, with high temperature occurred.

September 11: About seven weeks after the operation, the patient died with symptoms of meningitis, but the paralysis of the leg had practically disappeared.

A post-mortem examination revealed a cherry-red cyst projecting between the optic nerves into the anterior fossa of the skull, with the tail end of the cyst lying immediately over the opening made by the operation on the floor of the sella turcica. There was considerable absorption of the dorsum sellæ and basal meningitis.

The cyst, with microscopy, perimeter and temperature charts and skiagram, are shown.

Report of a Case of Sarcoma of the Pituitary Body treated by the Killian-Hirsch Operation.

By E. D. DAVIS, F.R.C.S.

A COMMERCIAL traveller, aged 23, complained of right frontal and temporal headache, at first spasmodic, and then continuous, of two years' duration.

July, 1912: The eyes were examined at an ophthalmic hospital and found to be normal. At a later date, an adenoid operation was performed for nasal discharge at a throat hospital.

February, 1913: Retro-bulbar neuritis was diagnosed. The right eye was affected first, then the left. The neuritis was considered to be caused by sinus suppuration, and a nasal operation was performed when apparently the left middle turbinal was removed, but no relief from the headache or nasal discharge was effected.

February, 1914: Sent home from Ceylon for frontal sinus operation, as headache was now continuous and worse.

June, 1914: Central optic atrophy and posterior synechia were detected and the patient was sent for an examination of the nose. The nose and nasal sinuses were normal, with the exception of absence of the left middle turbinal, and the drying of mucus in that position. Acromegaly was then diagnosed by the large hands, feet, lips, and jaw, and by his general appearance. He was also almost distracted by continuous headache. Pulse 80. Temperature 97° to 98·4° F. X-ray photographs of the large sella turcica, &c., taken before and after operation, are shown. Skeletal changes with ununited epiphyses were marked. Wassermann reaction negative. Lumbar puncture: Low tension, small lymphocytes in excess. No relief of headache. Urine normal. Sugar, tolerance test not done. Fields of vision: The perimetric charts are shown.

June 17: The Killian-Hirsch operation was performed with no relief, and later, increase in the loss of sight of right eye. Pieces of glandular pituitary with proliferating cells were removed.

On August 5 Mr. Percy Sargent attacked the growth by raising large parieto-frontal osteoplastic flap, including the removal of the roof of the right orbit. The brain was elevated, and an easy and clear access was obtained to a cherry-like growth projecting between the two optic nerves. Severe hæmorrhage occurred when an attempt was made to remove the tumour. Patient died the same day. The post-mortem specimen of the base of the brain is displayed. A piece of the large cystic growth projected into the sphenoidal sinus through the opening made at the first operation.

Post-mortem Specimens of a Malignant Growth of the Pituitary Body.

By E. D. Davis, F.R.C.S.

A laundress, aged 52, was admitted for symmetrical ophthalmoplegia of about eighteen months' duration. A pale, bedridden woman of slow cerebration, and with the appearance of a hypopituitary case with slight œdema of the upper eyelids. Complete paralysis of the third, fourth, and sixth cranial nerves on both sides. Pupils equal, of moderate size, with no reaction to light or to accommodation. Optic disks and sight normal. Weakness of both legs. Skiagram: Huge sella turcica and opaque sphenoidal sinuses. Nose and ears normal, but aspiration of the right sphenoidal sinus with a Watson-Williams syringe and straight trocar resulted in a syringeful of old and recent blood which suggested a growth. The Killian-Hirsch operation was considered inadvisable.

The specimens show a large soft growth of the pituitary body which has eroded the sella turcica and dorsum sellæ. The tumour has extended laterally and compressed the cavernous sinus and nerves, and has also burst through into the nasopharynx. The sphenoidal sinuses are filled with growth. Secondary growths are present in the omentum.

The skiagram and microscopical sections of the tumour are shown.

DISCUSSION.

The President said that when he had used the term "Killian-Hirsch operation" Hirsch rather objected, because Killian had never operated for a pituitary lesion. He (the President) used the term to indicate the route taken, as Killian's submucous septal operation was first performed preparatory to opening the sphenoidal sinuses and the anterior wall of the sella. It was thus sufficient to speak of the Hirsch operation in this connexion.

Mr. Howarth said that members would have been struck by the fact that these cases were usually shown as post-mortem specimens. He was, perhaps, fortunate in having operated some two years ago upon a case which was still alive. It was not generally the fault of the surgeon that so many cases were fatal, but was due rather to the fact that the cases were unsuitable for this particular operation. The operation of sellar decompression was a good one if there was a tumour in the sella turcica, below the diaphragma sellæ; but one was often largely dependent for diagnosis on the physician and the

ophthalmologist, and sometimes they asked one to operate in cases where the tumour was not in the sella. If the tumour was an interpeduncular one it would be above the diaphragma sellæ, and this operation would not relieve the condition. Still less would it do so if the tumour was a pontine one lying in the dorsum sellæ. If, however, one hit off a case of true pituitary tumour which was confined to the sella turcica, the operation was useful. His successful case was one of these. The pathologist who examined the piece of growth that was removed said that it showed a simple hyperplasia of the pituitary body. The patient was much benefited by the operation. He could recall three fatal cases, in two of which the tumours were interpeduncular and one in which it was pontine.

The PRESIDENT said he had recorded a case which died from hæmorrhage some hours after operation; post mortem it was found to be quite an unsuitable case for operation. One of Mr. Davis's cases was that of a cyst in the pituitary fossa and above the diaphragma sellæ, and that kind was often relieved by this form of decompression. Mr. Graham had operated upon a case which did very well for two months, but he had to do a second operation, and post-mortem examination showed it was not a favourable case for obtaining substantial decompression by the nasal route, owing to the amount of intracranial growth above the diaphragma sellæ.

Dr. SYME said Mr. Davis's "hard luck case" ought to have recovered, and he suggested that these patients should be kept in the sitting posture after operation, to allow of better drainage than could take place in the recumbent posture.

Mr. DAVIS replied that the cyst case was disappointing, for the cyst proved to be beyond his reach. At that operation one would have liked to have passed the finger into the cranial cavity.

Case of Laryngeal Obstruction following Specific Infection in a Young Man.

By W. H. KELSON, M.D.

THE patient, who is a meat salesman, was sent to the exhibitor by Mr. A. Shillitoe owing to his difficulty in breathing. He acquired specific disease in 1904, and was successfully treated for this by Mr. Shillitoe, marrying in 1907, and having three healthy children. His voice, however, which was affected in the early stages of his complaint, has never recovered; and this winter the breathing has been much embarrassed.

On examination, fleshy masses resembling pachydermia were to be seen just below the ventricular bands and in the inter-arytænoid space,

and the whole larynx was much congested. After cocainisation there was seen to be a good deal of subglottic swelling. After the use of vapor benzoin co. and alkaline lotion the swelling somewhat abated, but as the breathing is still affected the question arises whether any operative measure is desirable, and if so, of what nature.

DISCUSSION.

Dr. ADOLPH BRONNER suggested the use of "606." He had seen a marvellous improvement follow from it in a case in which the tonsil was similarly affected.

Dr. JOBSON HORNE considered the condition might be described as *pachydermia laryngis diffusa*. The condition was rare; a similar case had not been brought before them for a great many years. About twenty years ago, in conjunction with the late Professor Kanthack, he exhibited before the Laryngological Society of London the larynx from such a case, in which the pachydermatous excrescences were perhaps more diffuse and extensive, reaching to the trachea, and had caused death by suffocation. The description of the case would be found in the *Proceedings of the Laryngological Society of London*, May, 1895, ii, pp. 82-83. The specimen itself was placed in the Museum of St. Bartholomew's Hospital. The case before them was distinct from the cases of tertiary syphilis of the larynx with which they were all familiar, presenting gummatous infiltration and ulceration. Those of them who were practising laryngology before the days of salvarsan could call to mind the gratifying results obtained in such cases by iodide of potassium and perchloride of mercury without operative measures. In the present case there was not a loss but an increase of tissue, and it might be necessary to resort to surgical treatment.

Mr. WHALE said he showed a very similar case six months ago, and under "606" it cleared up entirely. He saw the woman a fortnight ago, and she was then well.

Mr. HARMER asked whether any member had, in these cases, tried the effect of intravenous injections of perchloride of mercury. It was suggested, some time ago, that by using 1 in 1,000 perchloride of mercury, dissolved in normal saline solution, and by giving 5 to 15 c.c. intravenously once a week, as good results were obtained in tertiary syphilitic conditions as those produced by salvarsan. This treatment had given good results in the few cases tried at St. Bartholomew's Hospital. It was an easy treatment, and perfectly safe.

Mr. E. D. DAVIS said he had had two cases of a similar kind. In one he had to perform tracheotomy, and then gave neo-salvarsan. There was a complete sequestrum of one arytænoid. The patient had two doses, and did very well. The other was a case of perichondritis, in which tracheotomy seemed inevitable; but he gave neo-salvarsan and the condition cleared up without the necessity for tracheotomy.

The PRESIDENT said he supposed all were agreed that in syphilitic cases surgical operation should be regarded only as a *dernier ressort*.

Dr. KELSON replied that he was grateful for the suggestions, and he would put them before the patient. Whether this condition was syphilitic or post-syphilitic was unknown. Wassermann's reactions had not been done recently.

Case of Fixation of Right Crico-arytænoid Joint; (?) Traumatic.

By W. M. MOLLISON, M.C.

J. W., AGED 27, attended in the Throat Department at Guy's Hospital in July, 1914, on account of a cough of a few days' duration, thought to be due to the inhalation of a lemon pip. There was no evidence of this at all, and he soon recovered.

The fixation of the right vocal cord and the very prominent and apparently displaced right arytænoid were, however, noted; the Wassermann reaction then as now (January, 1915) was negative. There is a scar in the neck, of an operation performed ten years ago for removal of a goitre. His hoarseness certainly dates as far back as the operation, but perhaps even antedates it.

DISCUSSION.

Dr. SYME said he exhibited a similar case before the Scottish Otological and Laryngological Society last May. It seemed to be congenital, and this case also appeared to be congenital. He asked how the tilting of the arytænoid could be accounted for. He found the same displacement in his own case. This could not be accounted for by traumatic paralysis.

Sir WILLIAM MILLIGAN asked whether Mr. Mollison associated this condition with the operation. He thought these cases were not very uncommon. He had seen a case in which the condition was bilateral. An operation was done many years ago, and one cord became paralysed. Then the other lobe of the thyroid was removed a few years afterwards, and paralysis ensued on the other side. The patient was in such a bad condition with abductor paralysis that he had to do tracheotomy. He did not know whether there was injury to the nerve at the time of operation, or whether cicatricial contraction ensued afterwards.

Dr. FITZGERALD POWELL said he had seen cases in which this condition had occurred in goitre previous to operation; the gland had become inflamed and adhesions had been formed, involving the recurrent nerve. He thought that fixation of the cords was not uncommon in goitres before operation.

Dr. JOBSON HORNE agreed with the previous speakers as to the frequency of fixation of a vocal cord in cases of goitre which had been operated upon; and he also agreed with the statement in the notes of the case that it, perhaps, antedated the operation. In this case he did not regard it as traumatic in origin. If it had been traumatic, one would have expected, after this lapse of ten years, to have seen some atrophy of the vocal cord and arytænoid affected, but in this case there was some enlargement and prominence of the arytænoid region of the affected side.

The PRESIDENT said this was the typical condition met with when there had been injury to one nerve either before or during a goitre operation: perhaps after about one in ten goitre operations there was some paralysis in the larynx. If one fact more than another was apparent, it was that in the traumatic post-paralytic condition there was contracture, in the same way that a fixed knee following paralysis got out of position. The last thing he would expect would be to find the arytænoid in a normal position after post-paralytic contracture. He had exhibited a case in which the joint was fixed and displaced in the same way. The contracture drew the arytænoid forward, so that, as in this case, the healthy side of the glottis was the longer. In the case now exhibited the arytænoid was, he thought, larger and more prominent than usual. In his own case removal of a portion of the arytænoid was performed, with rather good results; hæmorrhage prevented him getting the whole arytænoid away, however. Excessive bleeding was common in goitrous patients.

Mr. MOLLISON replied that he called it fixation because that was the condition. At some time the patient had had paralysis of the cord and fixation of the crico-arytænoid joint had taken place subsequently. He did not agree that the arytænoid was enlarged, but thought the appearance due to displacement. In examining a large number of normal larynges one now and then saw a slight degree of this displacement. Sir StClair Thomson, who had now left, wished him to say he looked at the case and thought it unnecessary to assume that the nerve had been damaged at the operation: it might have become involved in the scar after the operation. The hoarseness might have come on a short time after the operation. The voice was now good, and he had not contemplated any treatment.

Report upon Sir StClair Thomson's Specimen of Laryngeal Growth, from Case shown December, 1914.[1]

By S. G. SHATTOCK, F.R.C.S.

"In my opinion the specimen is an early squamous-cell carcinoma. The section marked × is the best. Besides the fact that the epithelium

[1] *Proceedings*, pp. 33-38.

takes the form of ingrowths, there are other characters to notice—viz.: The presence of cell-nests; the great size of many of the cells and their nuclei; the presence of more than one nucleus in many of the cells; the infiltration of Unna plasma cells about the lesion."

(Signed) S. G. SHATTOCK.

Pachydermia Laryngis.

By W. JOBSON HORNE, M.D.

AT the previous meeting of the Section of Laryngology, in the course of a discussion upon a case of epithelioma of the larynx and the microscopic sections of the growth removed, Dr. Jobson Horne remarked that the sections as seen upon the lantern screen, and which he had not seen under a microscope, reminded him of sections of pachydermia of the larynx, which he would be pleased to exhibit at a future meeting.

He now demonstrated these sections and pointed out that pachydermia of the larynx, as is known, may be *pachydermia laryngis verrucosa* or *pachydermia laryngis diffusa*. The former is the clinical condition more commonly known through the text-book descriptions, but the latter is of equal or of greater importance clinically as liable to simulate, or to be part and parcel of, the more serious disease, epithelioma of the larynx. *Pachydermia simplex* is therefore a condition to be eliminated in the differential diagnosis of cancer of the larynx.

Dyspnœa, Perichondritis, Tumour (?) of the Mediastinum.

By W. STUART-LOW, F.R.C.S.

A MAN, aged 53, was sent to the clinic on account of increasing difficulty of breathing, especially on exertion; for some weeks he had been liable to attacks of nocturnal dyspnœa. He was at once admitted as an in-patient, having had a dyspnœic recurrence of such severity that the necessity for tracheotomy seemed imminent. There was a fullness in the neck over the left side of the trachea, filling out the lower part of the carotid triangle, and a swelling over the left side of the thyroid cartilage, which was tender on pressure. The vocal cords were somewhat suffused, but the movements were normal. A livid bulging was observed

under the left vocal cord, extending downwards in the trachea. Dry cupping, applied over the left side of the thyroid cartilage, relieved the difficulty of breathing. This has been repeated twice daily for a fortnight. The exhibitor has recently employed dry cupping in a variety of laryngeal cases with advantage. The subglottic swelling disappeared under this treatment, and the swelling on the left side of the trachea was also reduced. The Wassermann test was found to be positive. Iodide of potassium and mercury have been given in increasing doses and neo-salvarsan intravenously on three occasions. A skiagram revealed a large mass in the chest. There are no indications of pressure on the phrenic or vagus. A second skiagram was taken a week ago (both skiagrams exhibited). It is very remarkable that there are no indications of pressure on nerves or veins, such an extensive growth being present.

DISCUSSION.

The PRESIDENT asked whether Mr. Stuart-Low had had advice as to whether the condition was possibly aneurysm, as there was decided pulsation, and the skiagram led one to think of that lesion; though he (the speaker) could show skiagrams suggesting aneurysm where the condition was really mediastinal growth, and vice versa. In mediastinal growth there might be communicated pulsation. In this case the outline in the skiagram was very sharp, and was, in his opinion, much more suggestive of aneurysm than of growth. The case ought to be shown again if there were further developments.

Dr. DUNDAS GRANT said the pulsation and the tracheal tugging strongly suggested aneurysm, but it was not absolute proof, as shown by a recent case in which tracheal tugging was very marked, but Dr. Inman had found the Wassermann reaction negative, and as a pathologist he held that this negatived aneurysm; in point of fact, it was a mediastinal new growth. In the present case the Wassermann reaction was positive. The complete absence of paralysis of the left vocal cord was an interesting hiatus, but this did not exclude the possibility of aneurysm.

Dr. DAN MCKENZIE, alluding to the difficulty of diagnosing pulsating tumours at the root of the neck, recalled a case which came under his care four or five years ago, a weak-minded girl, aged 15, with a tumour in the thyroid region of the exact contour of the thyroid gland, and pulsating. He thought it was goitre, and as it was causing trouble in swallowing, and there was also difficulty of breathing, he decided to remove it. It was easily freed from its connexions, and the capsule was smooth. On cutting through the capsule he found inside a dermoid cyst which extended down to the anterior mediastinum, and was adherent to the aorta, from which pulsation was imparted. He did not feel justified in attempting to dissect it out. The girl

got well from the operation, but naturally the tumour still continued to discharge dermoid material. It was a very rare condition, and rather a startling one to encounter.

Dr. FITZGERALD POWELL asked whether the two pulses were equal, and whether a pulse tracing had been taken.

Dr. KELSON said the man was evidently suffering from active syphilis, and he had what looked like syphilis of his thyroid. The tumour he regarded as a gummatous mass in the chest. The only point against that was the absence of response to anti-syphilitic remedies, though that did not put gumma altogether out of court. Absence of pressure signs with such a large mass was rather in favour of syphilis.

Dr. SYME suggested that abductor paresis existed in the case, on the right side.

Mr. STUART-LOW replied that Dr. Purves Stewart had examined the case most carefully, and was of opinion it was not aneurysm. Dr. Ironside Bruce and the physicians at Charing Cross Hospital expressed a similar opinion. There was no pressure anywhere, and it was described as an infiltrating neoplasm, of very slow growth. It could not be dermoid, because the patient was a hard-working man and had been at work until a few weeks ago. The two pulses were equal. It was generally thought it might be a gummatous infiltration, but it must be remembered that the man had had iodides, mercury, and neo-salvarsan without improvement. He had not heard of there being any impairment of vocal cords until Dr. Syme mentioned it. The larynx was now normal. He would report subsequent happenings.

Epithelioma of Pharynx and Tongue.

By W. STUART-LOW, F.R.C.S.

A MAN, aged 48, who came to the clinic complaining of a hard swelling in the neck, and pain in the throat and tongue of three months' duration. The middle and left sides of the base of the tongue were felt to be stony-hard, and the uvula, lower and left half of the palate, and left side of the pharynx, were thickened and firm to the touch. Dr. Wyatt Wingrave's report pronounced the growth to be a rapidly growing epithelioma. The infiltration and induration being so extensive as to make it inoperable, diathermy was resorted to, the first application being made on January 14. Six punctures with the thermic point were made—two in the palate, two in the lateral wall of the pharynx, and two in the base of the tongue. There was so very little reaction, and

the man was swallowing so much better five days after, that a repetition of these punctures was made a little way off from the original places of puncture. In a week, more punctures were made into those parts of the tongue and pharynx that felt the hardest to the finger. The cutaneous structures over the hard mass in the neck were incised and reflected, the hard glands were punctured in three places, and the skin incision stitched up.

The man says that he has less pain and is swallowing better.

Epithelioma of Left Tonsil and Adjacent Part of Tongue after Operation (Specimen shown).

By C. I. GRAHAM, F.R.C.S.

MALE, aged 56, noticed pain and difficulty in swallowing about mid-December, 1914. It was equally difficult to swallow solids and liquids, but solids produced more pain. He was admitted to St. Mary's Hospital on January 6 with an ulcer confined to the lower pole of the left tonsil and a hard nodule in the adjacent part of the tongue. There was a hard mass, the size of a walnut, at the level of the tip of hyoid beneath the left sternomastoid. There was no limitation of the tongue movements, and a No. 12 œsophageal bougie passed easily to the stomach. Pyorrhœa was well marked. Mr. Dolamore removed all the teeth, about twelve, under gas anæsthesia, on January 7, and operation took place on January 13. The left anterior triangle was dissected clean, including the posterior part of the digastric triangle. No vessels of importance were tied. The left cheek was then split, and the left faucial arch and a considerable portion of the tongue were cut away in one piece. The wound in the neck was completely sutured except for one drainage-tube at the lower angle, and another at the level of the hyoid. The cut edge of the tongue was united to the alveolar mucous membrane, and the gap in the tonsillar region was reduced by means of catgut sutures. There was very little hæmorrhage, in spite of the fact that no important arteries were tied, and the intratracheal administration of ether made the operation most comfortable. Laryngotomy was performed four hours after operation on account of dyspnœa. There is a sinus at the left angle of the mouth which is rapidly disappearing.

Epithelioma of the Tonsil.

By W. Jobson Horne, M.D.

The patient, a married woman, aged 43, for years on and off had had an uncomfortable stuffy feeling at the back of the nose and throat, getting no ease until "something moved." About five or six months ago the back of the tongue and throat ached and the throat was very dry, so the patient tried painting it with glycerine of tannin. A swelling on the left side of the throat was then discovered; this had got worse, causing pain in the tongue, throat and ear, and at times dysphagia. The left tonsil, which is enlarged, together with the anterior and posterior pillars, formed a red projecting mass with an ulcerating surface. The swelling felt very hard on palpation. The larynx was not involved.

The case was shown with a view of eliciting opinions as regards treatment, the patient being opposed to an operation.

DISCUSSION.

Dr. Dundas Grant asked whether in Mr. Stuart-Low's case the glands gave evidence of having contracted to any considerable extent under diathermy. If the method would produce that result without breaking down and suppuration it would be a very welcome addition to the means of treatment.

Mr. Harmer said he had never yet used diathermy for treatment of the glands. The cases now shown were interesting, because one saw, side by side, results after diathermy and after radical operation. He had no doubt that diathermy caused much less shock to the patient than a cutting operation, and recovery was more rapid. In regard to results, there was very little to choose. He believed, however, that diathermy would, in the future, when more was known about it, yield the best results. Dr. Horne's case he did not regard as a good one for diathermy, because it infiltrated deeply into the tissues of the neck. But the greater part of the growth could be destroyed, and the patient would have three to six months of happy existence.

Mr. Mollison said he had now a case undergoing diathermy very similar to Dr. Horne's, and he had been very disappointed in the results so far. Diathermy had been done three times, and, beyond the fact that the surface of the growth was now looking cleaner, there was very little change. Indeed, the extension downwards into the pharynx was now greater than a few weeks ago. But the glands disappeared under small doses of radium.

The PRESIDENT said it was often helpful to use both radium and diathermy in these cases, if these means were available. Results from diathermy seemed to vary with technique. Even if the small apparatus of Schall was used, it promised well in faucial conditions.

Dr. FITZGERALD POWELL said he regarded Mr. Graham's as a very successful case, and he did not see why the woman should not have the same treatment. If there were a deeper recurrence, diathermy could be used. If untreated, there would soon be a rapid extension.

Sir WILLIAM MILLIGAN said he had seen the injection of a few drops of pure formalin into a malignant tonsil give great relief in a case in which operation was declined. A large slough ensued, and the patient was comparatively comfortable for a considerable time.

Mr. STUART-LOW replied that the reduction in his patient's case was very considerable indeed. He made several punctures, moving the needle about in the tissues, so that a considerable area was treated. Next day it was decidedly softer, and the incision healed immediately after stitching. The patient felt much better for this treatment, the pain being less and the hard, stony glands being much softened and greatly reduced in size.

Mr. GRAHAM replied that his case was more suitable for removal by operation, and he thought that was the best method. He emphasised the remark made by Mr. E. D. Davis, in replying on his own case of naso-pharyngeal fibroma (p. 56), as to the extreme comfort experienced by the surgeon when the anæsthetic was administered by the intratracheal method.

Dr. JOBSON HORNE expressed his indebtedness to those who had made suggestions. His patient had definitely made up her mind not to submit to surgical procedures, and he had sympathy with that attitude in this case, as the growth, he thought, was really more extensive than it appeared to be. He would avail himself of Mr. Harmer's suggestion and advise treatment by diathermy.

Bullet Wound of Pharynx, &c.

By C. W. M. HOPE, F.R.C.S.

A. B., WHILST resting two miles behind the firing line in France, was shot in the neck on October 28 by a rifle bullet. He spat up a little blood at the time of receiving the injury. Entrance wound is behind right sternomastoid, 1½ in. above clavicle. The bullet was removed (by incision over it) just below angle of left lower jaw.

On January 8, 1915, marked brawniness of neck on each side of

larynx and upper part of trachea was noticed, and a hard mass was felt in the region of right transverse process of lower cervical vertebræ; nodding and rotatory movements of head were limited. Patient was only able to swallow fluids, and that with some difficulty. He had definite weakness of left vocal cord, which was narrower than the other, and had poor abduction.

On January 13, 1915, he was examined by direct method, under chloroform. The post-cricoidal pharynx was found to be obstructed by two nipple-like processes of granulation tissue, ¾ in. long and ¼ in. broad, growing from posterior wall. No abscess cavity or pouch could be demonstrated. The masses were removed by forceps. Within twenty-four hours patient was able to swallow solid food with ease.

January 22: Examined again under cocaine. Tiny tag of tissue found on posterior pharyngeal wall and removed. No stricture of lumen. Patient able to eat anything. Gained 8 lb. in weight in ten days. All swelling of neck subsided except over right transverse processes. Movements of head free. Vocal cord *in statu quo*.

Skiagram shown, kindly taken and reduced by Dr. Knox.

Specimen and Skiagram of a Case of Œsophagotomy.

By H. L. Whale, F.R.C.S.

Gunner S., drinking the gravy out of a bully-beef tin in the trenches, swallowed this circular piece of tin, which is 1⅜ in. in diameter, bent and jagged. It was impacted in the coronal plane opposite vertebra D_2. He came under my care in Boulogne eight days later. As his occupation in peace time is to play the oboe, it was necessary to avoid tracheotomy or laryngotomy. The œsophagus was exposed in the left posterior triangle of the neck, and opened. The mucous membrane was found folded around the jagged edges of the tin, making extraction very difficult. An œsophageal tube was passed, which the patient subsequently was taught to pass for himself for feeding purposes. This tube was used for ten days. The œsophagus was closed, and the outer wound packed. At the operation the cervical sympathetic and its middle ganglion were clearly exposed: and at several dressings the left pupil was observed to dilate when ribbon-gauze was being packed between the œsophagus and spine. Recovery was quite uneventful.

DISCUSSION.

The PRESIDENT said Mr. Whale was to be congratulated on the result.

Mr. O'MALLEY also congratulated the exhibitor on the result because of the difficulty of operating in this region. Two days ago he had witnessed an operation in which a bullet was removed from between the third and fourth ribs, and he appreciated how difficult it was.

Case of Gunshot Wound of the Neck; Injury to Larynx, resulting in Formation of Web.

By H. BUCKLAND JONES, M.B.

A PRIVATE, in the Oxford and Bucks Regiment, aged 28, was wounded at Ypres, on October 23, 1914, by a piece of shrapnel casing, about the size of the little finger-nail, which he coughed up two days later. There was dyspnœa for five or six hours; he was in pain for a week and coughed up clots of blood. On admission to the Metropolitan Ear, Nose and Throat Hospital on December 17 the case was investigated under the X-rays with negative results. On examination there is a scar on the right side of the neck over the sternomastoid opposite the cricoid cartilage. The glottis is closed by a crescentic web occupying its anterior half. The ventricular bands are somewhat thickened. Both arytænoids have equal and almost complete movement; possibly the left is a little more free than the right, except that adduction leaves a chink in the posterior one-third of the glottis.

DISCUSSION.

Sir WILLIAM MILLIGAN said he thought the patient would be best left alone; he doubted whether anything could be done successfully.

The PRESIDENT said he once dealt very well with a web by doing laryngofissure and keeping the fissure open by means of a special splint until the wounded edges of the cords healed up. He sutured the fissure three weeks after operation.

Dr. DUNDAS GRANT thought one would only feel called upon to operate if there were stridor or dyspnœa. He did not think the respiration was being interfered with in this case; though if laryngitis occurred now there would be more discomfort than in an ordinary person. Brünings suggested injecting paraffin into a paralysed vocal cord. He agreed that the web was best left alone for the present.

Mr. O'MALLEY said that eighteen months ago he showed a boy who had a web, and a full crop of suggestions for treatment was made at the meeting. He followed the active advice, but the result was unsatisfactory; considerable œdema ensued, and the web returned to its former condition. He dealt with it endo-laryngeally. He advised Mr. Jones not to take an active line in this case. With regard to the voice weakness, he asked that consideration should be given to an emotional factor.

Mr. WHALE thought it a suitable case for suspension apparatus, if anything were done. From what he had seen of similar wounds in France recently, he thought malingering was a possibility, especially if there was a desire to avoid getting back to the Front.

Mr. E. D. DAVIS said he had had two cases, both neck wounds, in which there was functional aphonia. The bullet had gone through the pharynx without injuring the larynx in both cases, but whereas one patient recovered the day after being seen, the other had not yet recovered his voice.

Sir WILLIAM MILLIGAN said he had seen, at the military hospital, three cases of functional aphonia in men who had had no injury at all.

Case of Gunshot Wound of the Neck, with Extensive Injury to Larynx.

By H. BUCKLAND JONES, M.B.

A PRIVATE in the Grenadier Guards, aged 20, was wounded at Ypres on October 21, 1914. He was standing up firing and was surrounded by the enemy, when he was hit in the neck by a bullet. He believes this entered near the tip of the thyroid cartilage and came out behind the sternomastoid on the right side. He was unconscious for about fifteen minutes and then walked to the field hospital. He had difficulty in breathing and speaking for about two days, when the latter improved to a low whisper. Some blood was coughed up for about twenty-four hours. He was admitted to the Metropolitan Ear, Nose and Throat Hospital late in December. An X-ray examination showed no sign of foreign body. There was a swelling at the level of the thyroid cartilage in the right anterior triangle, which eventually resulted in suppuration, and was opened and drained. On examination with the laryngoscope no vocal cords are to be seen; the regions of the ventricular bands are occupied by masses of granulation tissue, especially the right.

Discussion is invited as to the best method of treatment in this case.

DISCUSSION.

The PRESIDENT said he saw a similar case in a man from the Front. In such cases he supposed an expectant attitude must be adopted for a considerable time.

Mr. BUCKLAND JONES thanked members for their suggestions. In the first case he had thought of doing some endolaryngeal operation, but he gleaned that this would not be very hopeful. Perhaps he should try some voice-training method first. In the second case there was much perichondritis. Certainly there was some improvement after the abscess was drained, but there seemed to be much granulation tissue in the larynx. He proposed to do nothing for the present until the perichondritis had subsided.

Case of Shrapnel Wound of the Neck; Injury to Buccal Cavity; Exit beneath Angle of Jaw on Opposite Side.

By W. JOBSON HORNE, M.D.

THE patient, a Grenadier Guardsman, on November 7, 1914, at Ypres, was wounded by a shell in the left thigh, the occiput, and the left side of the neck. There was also the scar of a wound on the right side of the neck, which was regarded as the point of exit. The trench was blown in by the bursting shell, and the man was buried in the *débris*. He was unconscious for about twelve hours. He was in hospital in Boulogne when he recovered consciousness. The mouth contained blood, and dirt, and broken teeth. He was not able to swallow saliva, and during the nine days he was in Boulogne a feeder had to be used. Whilst he was in Boulogne pieces of shrapnel were removed from the wounds in the neck, at the sites of entry and of exit. From Boulogne he went to Manchester, where he was convalescing until December 19, and then was on sick leave until January 26. The following day he was sent to the Metropolitan Ear, Nose, and Throat Hospital on account of pain in the region of the wounds in the neck, and disease of the right ear. The wounds in the thigh and occiput had healed and gave no trouble. The injuries to the mouth might have been occasioned by the blowing in of the trench. The X-ray examination of the neck had not so far yielded any definite evidence of a foreign body.

Opinions upon the case generally, and upon further treatment, were invited.

Synechiæ in the Nose treated by Diathermy.

By Dan McKenzie, M.D.

The patient is a corporal in a Lancer regiment, who sustained a shrapnel wound of the face four months ago. The ball traversed the face from one cheek to the other, passing through the nose. During the healing of the wound the nasal cavities became blocked by masses of scar tissue, so that when he came under my care respiration was almost wholly conducted through the mouth. The poor outlook for such a patient, if treated by ordinary methods, led me to employ diathermy, the apparatus used being Schall's small variety. The result has exceeded all my expectations. There was an entire absence of reactionary swelling, and the cicatricial tissue has shown no sign of forming again. It is suggested that for synechiæ of the more simple and everyday kind diathermy might with advantage be tried.

DISCUSSION.

Dr. Pegler said he had just discharged from hospital a soldier who had also been wounded in the nose at Ypres. Deep scarring showed where the lower end of the nose had been almost entirely severed from above and re-attached by sutures (at Rouen); the right nostril was almost completely obstructed by circumferential contractions and thickenings, the left rather less so. As a submucous operation was not practicable, the old routine methods were adopted, and india-rubber tubes were afterwards inserted to maintain an airway. The result was good and permanent on the left side, but the right vestibule he hoped to further improve.

The President raised the point whether diathermy was justifiable in such a condition as this.

Dr. Dan McKenzie replied that he would like to add the word "my" before "ordinary methods" in the seventh line of the note. His experience of treatment of cicatricial adhesions in the nose was not good; whatever he tried, the adhesions seemed to form again, but the result in this case had been surprising, as there was no reaction. After cutting or cauterising operations on the nose there were always swelling and œdema, and sometimes granulation tissue formation. This man breathed now even better than he did shortly after the operation. Contrary to his expectation, there had been no necessity to interfere further, as there had been a subsequent slow removal of redundant scar tissue, as the result of coagulation necrosis—an important part of the diathermy process.

Three Children illustrating Acute Suppuration of the Nasal Accessory Sinuses.

By HERBERT TILLEY, F.R.C.S.

(*a*) O. H. B., AGED 13. When aged 7 patient had scarlet fever. During the last days of convalescence, pain and swelling commenced over right eye, accompanied by purulent nasal discharge on right side. Œdema and redness over eye continued. Incision in eyebrow, escape of pus; anterior ethmoidal cells diseased—these were curetted. Frontal sinus opened, but found healthy. Healing of wound by granulation. On December 9, 1914, patient seen again; both eyelids were closed by œdema of upper lids, the œdema extending to vertex. Temperature, 101·5° F. Pain on pressure, most acute over site of right frontal sinus. No nasal discharge. December 10: old incision opened. Remains of sinus filled with purulent granulation tissue. Sinus walls completely removed, also one fronto-ethmoidal cell. Wound left open and drained; recovery uninterrupted. Sinus seems quite obliterated.

N.B.—Acute frontal sinus suppuration at seventh year and long period of quiescence (six years) until recent recurrence.

(*b*) *Acute Suppuration of Frontal, Ethmoidal, and Maxillary Sinuses.*—R. P., male, aged 8, on December 21, 1914, at 5 a.m., complained of severe pain in and around the region of the left eye. Temperature, 102° F. December 23: pain in eye severe. Conjunctiva reddened. Small reddish, tender swelling below and in front of tear sac. Considerable œdema of left upper eyelid. Left nasal cavity inflamed and much muco-pus. Examination rendered difficult by septum being deviated to left. Upon transillumination left antrum seen to be slightly darker than right. Exploration through inferior meatus established presence of pus in antrum. Continuous hot fomentations ordered. December 25: eyelids quite closed by œdema of upper lid. All symptoms increased except temperature, which was normal. December 25 to January 10: symptoms all abated except the swelling in front of lachrymal sac, which was tender, red, and semi-fluctuating. Free discharge of pus from left nostril. January 11: under general anæsthetic, anterior halves of middle and inferior turbinals removed. Inner antral

wall removed. Anterior ethmoid cells broken down with curettes until lachrymal bone and os planum were reached. Frontal sinus ostium enlarged by author's small raspatory. It was not possible to establish any connexion between external swelling in front of lachrymal sac and interior of nose. After operation fomentations were continued. The patient has made a complete recovery and is now practically free from any discharge.

The exhibitor thinks this may be the youngest case in which intranasal opening of sinuses has been carried out. Great difficulty was experienced during operation because of deviation of the septum. Killian's speculum had to be employed all the time while the higher sinuses were being dealt with.

(*c*) *Acute Suppuration in Maxillary Antrum following Measles.*—S. R., aged 10, female. January 24, 1913: Patient had measles. February 14: Exhibitor saw patient in consultation, when she was suffering from severe pain over left eye, in which region there was marked tenderness on pressure. Temperature, 102° F. Pain on pressure over left canine fossa. Free discharge of pus from left nostril. Nasal mucosa very red in left nasal cavity. Left antrum appeared dark upon transillumination. Exploration yielded a quantity of greenish-yellow pus. No indications of pus from higher cavities; the frontal sinus was irrigated. Irrigations of antrum on four following days, the purulent discharge rapidly giving way to a preponderance of mucus. March 31: Exhibitor last saw patient, when all symptoms had disappeared.

DISCUSSION.

Dr. WATSON-WILLIAMS said Mr. Tilley had done a service in bringing forward these cases, because they emphasised the fact that, even in children, one could treat these extensive sinus conditions. In the first, though there was no dead space and the condition healed up, there was a recurrence. Surgeons sometimes put down to a "dead space" what might be a re-infection. One saw that the surgeon was not always to blame, for there might be a re-infection from a latent condition. With regard to the intranasal method, it must be remembered that in very young children there might be an opening direct into the frontal sinus from the middle meatus, because the space formed by the development of the ethmoidal cells did not intervene as in adults. He did not think anyone would deliberately open the frontal sinus in such a child unless a skiagram were first taken, and even then it would be risky by intranasal methods.

Mr. STUART-LOW said but little pus was now to be seen, and the treatment seemed to have been beneficial; but he took exception to the amount of deformity in one of the cases. In a number of cases he had himself shown he attributed the absence of marked deformity in which this operation had been done to the wearing of a cage which took off the pressure. The use of this cage might have helped in the case of the boy, in whom there was very great deformity, which would, he considered, be permanent.

Dr. SYME said he was not convinced that it was advisable to operate on a child's antrum by the intranasal method, because he had found there was difficulty, even with the ordinary operation in children aged from 10 to 12, in keeping the opening into the nose patent. Antral cases in children should, he thought, be attacked from the canine fossa.

The PRESIDENT said he did not think Mr. Tilley had carried out more operative interference than the cases demanded.

Mr. HERBERT TILLEY replied that Mr. Stuart-Low's argument would not apply to this case, because the child he had referred to had no external dressings beyond lightly applied warm fomentations. In Case (*a*), when, six years ago, the sinus complication followed scarlet fever, it was an ethmoidal suppuration —there was a bulging into the nose and an external abscess. Mr. Hett opened it, and he only explored the frontal sinus, but, finding it healthy, he did no more. When he (the speaker) saw the case in December the œdema extended from the upper eyelids to the parietal suture, and there was much tenderness over the right frontal sinus. He re-opened the former incision and obliterated a diseased frontal sinus and an orbito-ethmoidal cell. In Case (*b*) he operated by the intranasal method because when opening through a child's canine fossa one destroyed the nerves supplying young teeth. There was no difficulty in operating through the nose, except that the septum was deflected. Neither had he any particular difficulty in entering the frontal sinus. In the earlier days of the disease he fomented the eye and forehead for ten days, but later, when the temperature went up, pain returned, and there was a recrudescence of the swelling in front of the eye; he considered that something more radical should be done in order to establish free drainage.

Tuberculosis of Nasal Fossæ.

By C. I. GRAHAM, F.R.C.S.

A FEMALE, aged 16, who had never had any nasal symptoms, nor any other illness, noticed slight nasal obstruction early in December, 1914, and she was seen at Christmas. There was a slight enlargement of the anterior end of the right inferior turbinal, which was red and granular

in appearance, with a slight amount of clear sticky discharge. The left inferior turbinal presented the same appearance, but in a less degree. A small piece was removed from the right side for examination. She was again seen on January 28 when there was definite ulceration of the right vestibule involving the outer wall, ventricle, and a small area of the adjacent septum. The left inferior turbinal remains practically stationary.

DISCUSSION.

Dr. PEGLER said he hoped Mr. Graham would add the statement that tubercle bacilli had been found in the tissues, and congratulated Mr. Graham on his success.

Mr. GRAHAM replied that the credit of finding the tubercle bacilli was due to Dr. Kettle.

Tuberculous Ulcer of the Mouth.

By H. BUCKLAND JONES, M.B.

W. M., A MALE, aged 30, came to the Metropolitan Ear, Nose and Throat Hospital, on December 31 last, complaining of ulceration of the mouth which he had been troubled with on and off for twelve months. On examination a large ulcer with indurated edges was found in the region of right inferior wisdom tooth admitting the tip of the finger and spreading on to the adjoining cheek. Some enlarged glands were found on both sides of the neck in front of the sternomastoid. There was a history of a cough and wasting for twelve months. On examination of the chest there was slight dullness over the right apex with prolonged expiration, some bronchophony, and a few coarse crepitations. There was no history of syphilis.

A piece of tissue was examined by Dr. Leathem showing numerous giant cells, but no tubercle bacilli. No sputum has been available for examination.

There is a microscopic section of this case.

Unfortunately this patient died of tuberculous meningitis a few days before the meeting.

Laryngological Section.

March 5, 1915.

Dr. JOBSON HORNE, Vice-President of the Section, in the Chair.

Angeioma (Bleeding Polypus) of Nasal Septum.

By WILLIAM HILL, M.D.

FEMALE, aged 25. A vascular growth, which has bled on several occasions, is seen springing from the anterior end of the right vestibular surface of the nasal septum. It has been noticed for six weeks, and is increasing. Eradication by surgical diathermy was proposed.

DISCUSSION.

The CHAIRMAN regretted that their President, Dr. William Hill, had been unavoidably prevented from presiding that afternoon. The nature and treatment of "bleeding polypus" of the septum of the nose was familiar to them all. The situation of the growth, in Dr. Hill's case, on the most anterior part of the vestibular surface of the nasal septum was perhaps a little unusual. The case reminded him of one which he had brought before the Laryngological Society of London many years ago.[1]

Dr. PEGLER agreed with the President's diagnosis; as sometimes happened with these angeiomas, the growth started from the septal wall of the vestibule close to the junction of skin and mucous membrane. He hoped that Dr. Hill would preserve the specimen by removing it entire in the usual manner.

Report upon Dr. Hill's specimen of septal angeioma.—The growth, somewhat mushroom-like in shape, cut vertically, belongs to the close-textured, soft cell type of *fibro-angeioma*. The convex surface is overlaid by fibrinous plasma containing leucocytes. Around the base is stratified epithelium sending in many prolongations of prickle cells. Thick fibrous trabeculæ ramify into the body of the growth from the pedicle, and merge in the dense meshwork of connective tissue sustaining endothelioid cells and enclosing abundance of blood sinuses, mostly very small. Lymphocytes have freely infiltrated an extensive area towards the periphery.—L. H. PEGLER.

[1] Vide *Proc. Laryng. Soc. Lond.*, 1896, iv, pp. 31, 32.

X-ray Photographs illustrating the Thoracic Appearances in a Case of Left Recurrent Paralysis associated with Mitral Stenosis.

By L. H. Pegler, M.D.

The patient (deceased) was a Jewish girl, aged 19, who came to the Metropolitan Throat Hospital in July, 1914, complaining of hoarseness on awakening and vocal disability which had dated from the previous January. This was found laryngoscopically to be due to paralysis of the left vocal cord, which was fixed. Dr. T. D. Lister, at Mount Vernon Hospital, had noted some dilatation of the heart, with presystolic and systolic murmurs. She was admitted at the Brompton Chest Hospital on August 11, under Dr. Bosanquet, who has kindly lent the photographs shown, and supplied the following notes taken during her stay, which terminated at the friends' desire on October 26. She died at home four days after her discharge, her local doctor reporting that she gradually sank.

On admission to Brompton Hospital the girl, who had suffered formerly from rheumatic fever, complained of cough, pain in left breast, and shortness of breath. A long presystolic murmur was audible at the apex, with thrill. Posteriorly, some râles at the bases of both lungs. The temperature remained at about 100° F. all the time; there were no tubercle bacilli in the scanty sputum. It was thought that some latent infection might be present as suggested by the fever. Dr. Dundas Grant examined her by direct bronchoscopy, and found no abnormal pulsation. Dr. S. Melville X-rayed the thorax on August 17 and reported: "Heart enlarged, purse-shaped. Above the left border is a well-defined shadow more or less in situation of right ventricle. Aorta wider than normal." In the absence of an autopsy, the intimate relationship between the cardiac conditions and the recurrent paralysis must remain a matter for speculation. Sir StClair Thomson in his book quotes Ortner[1] as the first to describe this rare complication of mitral disease, and states that thirty-seven more have been published since, but the instances are few in which the causal relationship has been established post mortem.

[1] *Wien. klin. Woch.*, 1897, x, p. 753.

DISCUSSION.

Dr. PEGLER wished to add that Dr. Stanley Melville, who X-rayed the thorax, now held the view that there was a mediastinal neoplasm. There was a number of blotches at the root of the lung in the skiagram, and these had suggested that diagnosis. Mr. E. D. Davis had just told him of a case he had treated in Charing Cross Hospital which had ended fatally, and in which mediastinitis had been found post mortem ; enlarged mediastinal glands were present, and Mr. Davis had formed the opinion that the recurrent nerve had been stretched by these bodies, and not by the dilated aorta. There was a good deal of recent literature upon the subject, and according to some authors it had been very difficult to clear up the actual relationship between the heart affection and the paralysis in several of the cases, even after a post-mortem examination had been made.

Dr. DE HAVILLAND HALL said that in the discussion of some months previously he brought forward a similar case, and he had known several other cases in which mitral stenosis was associated with left recurrent paralysis. Unfortunately it was not possible to verify the result in his own case, as in other cases, by necropsy. So far as X-ray examination went, his own case was undoubtedly due to pressure upon the left recurrent, owing to the dilated condition of the heart.

Sir STCLAIR THOMSON said that his experience of this condition was only second-hand, but he had read up the literature of the subject. There were few if any cases in which the post-mortem had proved that the enlarged left auricle was the cause of the paralysis. It would be a very good subject for exact investigation by young laryngologists with material. Killian had written, since his (the speaker's) book was published, to the effect that in his opinion it was anatomically impossible for the left auricle to produce any pressure on the recurrent laryngeal.

Dr. DUNDAS GRANT had seen this case at Brompton Hospital, and had sought to determine whether there was any evidence of pressure on the œsophagus and on the air passages, on the part of the left auricle, as revealed by œsophagoscopy. No abnormal pulsation was to be made out in the œsophagus ; certainly there was no pressure there. The bronchoscopic examination in this case was rather indecisive. In any case, the question of a direct pressure on the laryngeal nerve by the left auricle seemed to be very uncertain. They knew, from the relation of the left auricle to the aorta, that there was between the two the pulmonary artery—a very large structure, which would act as a kind of cushion separating them. The probability was that there had been some coincidental neuritis, perhaps associated with pericarditis, which was almost always present in extensive dilatations of the cavities of the heart. There was on record a case in which hoarseness disappeared after two days on digitalis, but in this particular instance, in spite of treatment

in bed and the administration of digitalis, there was no disappearance of the paralysis of the left vocal cord. The question as a whole seemed to be undecided, and he only regretted that in this particular instance they did not have an opportunity of clearing up the case, as Dr. Pegler would have wished, by means of an autopsy.

Mr. STUART-LOW recalled the case of a girl, aged 19, who had been very thoroughly examined by Dr. Hawthorne, and who reported on the condition of the heart. In this case the auricle was very large indeed, and he considered that pressure was direct on the recurrent nerve. In this particular instance, a long rest in bed had been effective, and movement of the vocal cord had partially returned under this treatment.

Specimens of Superior Maxillæ, exhibiting Abnormal Conditions of the Nasal Fossæ and Maxillary Antra.

By L. H. PEGLER, M.D.

THE exhibitor is indebted to Dr. Macphail, of St. Bartholomew's Hospital Medical School, for showing him, and permitting him to bring forward No. i, and, similarly, to Professor Symington, of Queen's University, Belfast, for Nos. ii, iii, and iv. They are anatomy room specimens, and therefore without histories.

(i) *A left side superior maxilla, from an aged female subject.*—[This specimen was shown at the December meeting.] There is a linear congenital deficiency in the outer wall of the inferior meatus, running nearly the whole length of the floor, and on a level with it. This morphological change is rare, and, according to Professor Keith, its correct interpretation is to be found in conjunction with a study of the upper jaw of a catarrhine monkey of the baboon class (a bisected skull of a baboon, and also of a chimpanzee, lent by the Royal College of Surgeons' Museum, and an illustrated paper by Professor Keith upon it, were shown [1]). The abnormality should be considered as a distension outwards of the inferior meatus, that has done duty for, though not homologous with, the maxillary antrum in this subject. In this light the true antrum in this subject is represented by a small chamber in the malar apex of the cavity, cut off from it by a bony partition, and communicating through a normally placed ostium with the middle meatus:

[1] *Journ. of Anat. and Phys.*, 1902, ii, pp. 47-50.

this is the normal location of the maxillary antrum in the baboon. It is somewhat puzzling to understand what has been gained from a physiological standpoint by the closing in of this well-placed drain aperture in the outer nasal wall, a change which in the ascent through the anthropoid apes—gorilla and chimpanzee—to man, has gradually evolved. The middle turbinate in this specimen is enormously hypertrophied and cystic.

(ii) *The superior maxillæ with nasal septum (detached) of a subject showing membranous adhesion of the inferior turbinates to the nasal floor.*—On both sides rather more than half the central part of the lower border of the inferior turbinate is fused with the mucous lining of the inferior meatus, establishing a supplementary channel. In consequence of the outer and anterior direction of the adventitious wall, the front aperture of this channel is contracted and much lateralised externally, but the posterior end is larger and points directly backwards. For purposes of demonstration, the left maxilla has been divided coronally into three parts, and shows the relations of the synechia, which Dr. Malcolm, of Belfast, in a well-illustrated paper on this specimen,[1] states to be 4 mm. thick and made up of mucous and submucous tissue; this structure might not impossibly be inflammatory, but the close similarity of the two sides does not favour this view.

(iii) *The superior maxillæ with portions of adjoining bones, from a male subject, aged* 62. — The two specimens, which are remarkably similar, show free communication between nasal fossa and antrum. This is effected through a round opening, about the diameter of a sixpenny-piece, with membranous margin, in the centre of a large shallow depression involving almost the whole of the outer nasal wall, and incorporating the normal ostium. Fibrous string-like bands of tissue slightly contract the right opening, and on the left side the vestige of middle turbinate that remains is adherent to the wall by radiating bands of a similar nature. A remnant of the anterior extremity of the inferior turbinate is seen in the left fossa. There are good evidences that the destruction has been wrought by syphilis.

(iv) *The lower part of the head and face, with roof of mouth and soft palate of a male, aged* 21.—Separation has been made from the upper part through a line carried below the orbits in a plane parallel with the

[1] Read before the Ulster Medical Society, April, 1914.

central horizontal axis of the septum nasi, and met by a coronal cut through the nasopharynx. Professor Symington states that the man had slight mental weakness. The palate is arched and shows a congenital median aperture, part of a fissure which in the soft palate has been closed by sutures. The cicatricial tissue suggests the existing displacement of the upper lateral incisors behind the central, following operation; the bicuspids are also slightly out of place. The nasal cavities are ill-developed and extremely narrow in the plane of section, which exposes a right-sided angular deflection of the septum. The posterior part of the middle and inferior turbinates are bulbous and hypertrophied, encroaching much upon the space, and recalling familiar abnormalities of lymphoid and mucous structures in this region, in cleft palate subjects.

DISCUSSION.

Mr. HERBERT TILLEY said his knowledge of comparative anatomy was not sufficient to enable him to discuss the specimens shown. The only clinical condition which somewhat resembled Dr. Pegler's first specimen occurred in a patient in whom a chronic left-sided antral empyema failed to be cured after the radical operation through the canine fossa. A second operation led to the discovery of a cavity occupying the malar recess, which communicated with the main antral cavity by a very small opening. When this recess was freely drained the suppuration ceased.

Dr. PEGLER said that the baboon-like antrum was being discussed that afternoon by the Anatomical Society, but some excellent drawings by Dr. Macphail, which he was able to show that afternoon, would recall the condition to those members who had seen it in December. It was destined for the Museum of the Royal College of Surgeons, and would always be available there for inspection, as would also the illustrations in the Anatomical Society's Journal.

Case of Synechiæ and Contraction of the Vestibules.

By L. H. PEGLER, M.D.

PRIVATE H. H., aged 25, Grenadier Guards, was wounded in the nose whilst entrenched at Ypres, by the bursting of a shell. The condition, when he came to hospital, was mainly one of stenosis of both nostrils, the end of the nose having been stitched up after being nearly severed, and a higher wound on the lateral aspect healed, whilst in France. The vestibules were almost closed by adhesions, and the

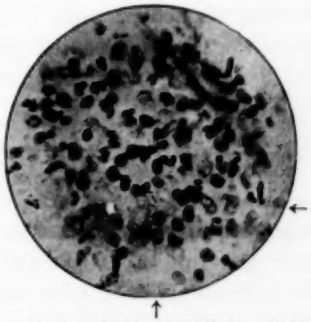
columella thickened and shortened by inflammatory deposits. The same influence had affected the turbinals, which had probably been previously hypertrophied, so that the fossæ were also much stenosed and nasal breathing prevented. According to the soldier's account his speech was altered by the accident, and so remains, excepting that much of his nasal resonance has been restored.

Treatment consisted in paring away cartilage and sawing off bony projections from the septum by the older methods, aided by the spoke-shave and punch, submucous resection being impracticable. Thus a way was cleared for reducing the right middle and left inferior turbinate in the usual manner, and for an india-rubber tube of about $\frac{2}{5}$ in. calibre being inserted in each nostril. These were worn continuously for a month, but on removing them, vestibular contraction from re-growth of tissue threatened to recur. It was not until two lesser operations had been performed, and the tubes had been worn for a few more weeks, that a good permanent airway became established. One would have liked to improve the external appearance of the nose, but the scars are becoming less and less conspicuous, and the patient is returning to duty breathing quite satisfactorily.

Case of Tuberculous Inferior Turbinate.

By Cecil Graham, F.R.C.S.

Mr. Cecil Graham showed a photomicrograph of the tubercle bacilli in one of the sections, and said that he took the material away from the nose a few days after he showed the case at the last meeting



Photomicrograph of tubercle bacilli in section from case of tuberculous inferior turbinate. By continuing the direction of the arrows the bacilli will be found at their point of junction.

of the Section;[1] Dr. Kettle then injected the material into guinea-pigs. The injection was made three weeks ago, and up to the present the guinea-pigs seemed to be perfectly healthy. The Section would have the result of the test before them at the next meeting.

Pathological report upon the case of tuberculous inferior turbinate shown at the last meeting by Mr. Cecil Graham. By E. H. Kettle, M.D. —" Sections show active tuberculosis. Numerous small foci are present, consisting of proliferating endothelial cells with occasional giant cells. In places there is some necrosis. A few bacilli are present, which in shape and staining reaction resemble tubercle bacilli."

The CHAIRMAN said, in view of the fact that at times acid-fast bacilli resembling tubercle bacilli, but of a non-pathogenic nature, were found in nasal secretions, a diagnosis of tuberculosis might be vitiated by depending solely upon the microscopic evidence. He was therefore glad to learn from Mr. Graham that in this case animal experiment had been resorted to. The result of the inoculation of the guinea-pigs must be awaited before the all-important point whether it was or was not a case of primary tuberculosis of the nose could be definitely established. Whatever the result of the inoculation might be, the completed case would be a valuable addition to the *Proceedings* of the Section.

Bullet Wound through Face; both Antra and Septum perforated; Operation.

By G. SECCOMBE HETT, F.R.C.S.

CORPORAL M., 1st Dorsets. Bullet wound of head on December 1, 1914. First field dressing applied at once. Injection of anti-tetanic serum. At the time patient was rendered unconscious; both eyes swollen up; teeth loose. Arrived in England complaining of headache, discharge from nose, and left-sided deafness, stiffness of jaw, and inability to open his mouth.

On admission to hospital on December 10 there was an entrance wound over the left malar bone 1½ in. anterior to auditory meatus, and an exit wound through the right cheek. The left lower eyelid was œdematous. The nose was full of crusts and pus, and there were adhesions between the anterior ends of both inferior turbinals and the

[1] *Proceedings*, p. 77.

septum, which were separated by Mr. Herbert Tilley. On December 15 a sequestrum was removed from the left nasal cavity, probably from the inner antral wall.

A skiagram showed a number of tiny metallic filaments (probably of nickel casing) along the track of the bullet. On transillumination and X-rays both antra were dark, but this was more marked on the left side. The photographs further showed fractures of the left malar, of the first upper molar on the left side, and of the first upper bicuspid on the right.

On January 14, under anæsthesia, the nose was explored, a perforation found involving the vomer, and sequestra were removed. The left antrum was opened anteriorly and found to be full of pus, granulations, and splinters of bone. The Caldwell-Luc operation was completed and portions of both inferior turbinals removed. Splinters of bone were also found in the exit wound. The jaw was opened by means of a gag gently applied.

Local treatment to the nose has been subsequently employed for the crust formation. The bullet, after passing through the face, embedded itself in the clay at the back of the trench, from which it was extricated by patient's brother. The core had evidently separated after the bullet's exit, from the nature of the wound, and only the casing was found. This casing is shown, and it will be noted that the point is only slightly blunted. The range is said to have been only seventy yards, and the patient is sure that the bullet came through a sandbag.

Patient, skiagram, and bullet were shown.

Mr. SECCOMBE HETT said that this case was of great military interest. It was an extraordinarily neat wound, made at a 70 yards distance, and one felt that the bullet must have gone through with its core intact. Five openings had been made through the bone, each opening being of a size only just large enough for a bullet to pass through. The malar bone was fractured. Mr. Hett described by means of a skiagram the track of the bullet, which, he said, was so perfectly straight that a probe could be passed from entrance to exit. The patient had a good airway through the nose.

Bone Impacted in Left Bronchus for Six Months; Removal; Recovery (Bone shown).

By Sir WILLIAM MILLIGAN, M.D.

HISTORY as given by patient: "On July 29, 1914, whilst at luncheon I felt a piece of bone slip into my throat. It appeared to go into the windpipe, and caused great difficulty in breathing, with gasping, coughing, and sickness. Relief in about a quarter of an hour, but had frequent attacks of coughing during the afternoon. Went to Scotland on August 1. During the month of August the cough continued intermittently, and was always worse at night. On one occasion, after cycling uphill, I had a severe attack of breathlessness and wheezing, and was quite unable to speak. Towards the end of the year I got very quickly out of breath, and on one occasion, after running to catch a car, breathed with great difficulty and in gasps. On January 19, 1915, I had a very violent attack of breathlessness after running upstairs, followed by cough, expectoration of blood, and pain in the back, shooting from the back of the chest to the front. I had to sit up for hours at night to stop the cough, and for the last three nights before the operation could neither lie upon my back nor right side on account of severe pain accompanying every attack of coughing."

X-ray examination upon January 21 (Dr. A. E. Barclay): "Slight thickening at root of left lung; no evidence of any bone."

Examined by Professor George Murray, January 23: "Breath sounds near left bronchus similar to the sounds produced by a large goitre pressing on trachea; no pulmonary disease."

Admitted to surgical home on January 27. Examination and removal of bone (per oral bronchoscopy) the following day. Complete recovery.

Subglottic (? Tracheal) Growth; Removal; Recovery.

By Sir WILLIAM MILLIGAN, M.D.

PATIENT, male, aged 66. Suffered from slight respiratory obstruction and intermittent attacks of aphonia from four to five years. No pain or expectoration. Respiration gradually becoming more difficult. Pedunculated growth seen arising from middle line of anterior tracheal

wall; attachment about ¼ in. below vocal cords. Both cords normal. Growth occasionally seen to swing up between vocal cords.

Removal by the "direct" method. Application of liq. ferri perchlor. fort.

Microscopic section was shown.

DISCUSSION.

The CHAIRMAN said that he had one remark anent the case to make on behalf of their President, who, although not able to be with them that afternoon, had, nevertheless, studied the notes of the cases brought forward. Dr. Hill raised the question whether the situation of the growth justified its being described as tracheal. The note stated that the growth was attached a quarter of an inch below the vocal cords. It would be better, in Dr. Hill's opinion, to describe it as a laryngeal and not as a tracheal growth. With that opinion he (the Chairman) fully concurred. In the literature, tracheal growths were comparatively rare, but the more common laryngeal growths were not infrequently subglottic in their attachments.

Sir FELIX SEMON asked what was the nature of the growth.

Dr. KELSON asked whether the growth was connected with the thyroid gland. Some three years ago he showed before the Laryngological Section a case in which the growth consisted of thyroid tissue.[1] It was well known that growths in that region were sometimes of this nature, and they were of interest developmentally.

Dr. DAN MCKENZIE said that subglottic growths were not uncommon, but pure tracheal growths were extremely rare. Some two or three only had been published in the *Journal of Laryngology* within the last few years.[2] The question was whether this was purely a tracheal growth.

Dr. DUNDAS GRANT asked what were the dimensions of the growth.

Sir WILLIAM MILLIGAN said that with regard to the question of calling the case a growth in the trachea, there was certainly something in the criticism which had been made from the Chair. He had thought of calling it a subglottic growth, but the attachment was fairly large. Whether the growth absolutely originated in the trachea, or was just above the origin of the trachea, it was extremely difficult to say. But he was quite willing to alter the title and call it a subglottic growth, although he thought part of the pedicle was attached to the mucosa of the trachea. With regard to the nature of the growth, he did not think it contained thyroid tissue; according to the microscopic examination it was a local lymphangiectasis with a fibrous capsule. As to its dimensions, it was about the size of a small marble, or, if this was too indefinite, say just under ½ in. in diameter.

[1] *Proceedings*, 1912, v, pp. 106, 137.

[2] *Journ. of Laryng.*, 1912, xxvii, pp. 123, 322; 1913, xxviii, pp. 376, 509, 596.

Dentigerous Cyst of the Lower Jaw; Operation.

By Sir WILLIAM MILLIGAN, M.D.

BOY, aged 15. Indefinite history of pain upon right side of face. Six months previously progressive swelling of right side of face; more marked recently. Slight pain, especially at night. Examination with finger in mouth causes pain and a definite papyraceous sensation.

X-ray report: "It is one of the most perfect cases of dentigerous cyst I have ever seen. It is very extensive, and in the lower part is a molar tooth. The bone is expanded to a thin shell, but there is no indication that the condition is anything other than an uncomplicated dentigerous cyst."—(Signed) A. E. Barclay.

Opening of cyst and removal of lining membrane (section shown), and tooth.

Photograph, X-ray photograph, and lower molar tooth were shown.

DISCUSSION.

[*Note.*—This case produced an interesting discussion on the treatment of dentigerous cysts of the upper jaw.]

Mr. HERBERT TILLEY said that cysts in the upper jaw as large as the one described could not be cured by simply scraping them out, because the granulation tissue nearly always suppurated, and this entailed a very long convalescence. In such cases he had always adopted the method of breaking into the antrum, removing the cyst wall entirely, making a large communication into the nasal cavity, and sewing up the buccal wound at the time of operation. If one opened these large cysts and packed the bony recess daily, in the hope that the cavity was going to granulate, the after-treatment would be a prolonged business.

Sir STCLAIR THOMSON supported Mr. Tilley's views. The treatment of cysts of this type in the upper jaw was most unsatisfactory. Two of his cases never got well. There were cases in the clinic for months and years, in which there was a continual history of stopping, and scraping, and waiting for this granulation which never filled it up. Therefore he had adopted the plan of trying to make them communicate either through the antrum, or directly, into the nose, and even then he had not been able to cure the suppuration.

Dr. DAN MCKENZIE said that he had been very much interested in hearing these remarks, because in a recent case which he saw there was an enormous

cyst which occupied the greater part of the antral cavity. In the course of the operation he accidentally broke through the small casing of shell-like bone, and having done that he obliterated it, and made communication in the way that had just been suggested by Sir StClair Thomson. He was very glad to hear that he had actually hit upon the right method. The suppuration ceased promptly, and the patient got quite well except that, from the distension of the antrum and the superior maxilla, it was impossible to keep closed the opening between the mouth and the antrum. The dentist had now made a plate which closed the opening. The speaker had tried to do a plastic operation, but the stitches broke down, the traction being too great.

Mr. H. L. WHALE asked how this growth had been approached—through the alveolus or the antrum wall?

Mr. TILLEY, replying to Mr. Whale, said that it was his practice to make an incision into the mucous membrane, over the most prominent portion of the cyst, to reflect this carefully, and then remove all the cyst wall, before reuniting the lips of the original incision of the buccal mucous membrane.

Sir WILLIAM MILLIGAN said that the operation took place two or three weeks ago, and absolutely the whole of the cyst wall was taken out; so far the case was going on satisfactorily. There was already contraction to the extent of at least two-thirds of the original size. He certainly looked forward to a good recovery. Had the cyst been in any way connected with the upper jaw or antrum, his treatment would have been different.

Perithelioma of Maxillary Antrum.

By H. L. WHALE, F.R.C.S.

MDLLE B., aged 52, Belgian refugee, has had an increasing lump under the right eye for four years. She came to me at the Hampstead General Hospital in September, 1914. The swelling then was limited to the antrum, bulging chiefly outwards and inwards, hardly at all towards the orbit, and not at all downwards. Puncture revealed a solid tumour. Chloroform being administered, the antrum was approached from under the cheek; the intention was merely to remove a piece for examination, but a large mass of growth was shelled out, and finally the appearance was almost normal. However, exhibitor felt sure that some growth remained at the upper margin. The mass now seen at and below the inner canthus has grown since then; but exhibitor, immediately after the former operation, went to France and lost sight of the patient.

Specimen, microscopic section, and skiagram were shown.

Histological report by Pathologist (Mr. H. Perkins).—A somewhat friable tumour, studded throughout with many small spicules of bone. In section: in parts the tumour presents irregularly circular areas, richly cellular, separated from one another by delicate fibrous tissue in which particles of bony structure are found. These cellular areas present internally a roughly circular space filled with red blood corpuscles. Bounding the space externally is a layer of flattened cells, suggesting the internal lining epithelium of a vessel. External to this are many closely packed small ovoid cells with a well-defined nucleus, arranged in a concentric manner around the lumen. In other parts of the section irregular columns of similar cells are seen infiltrating the fibrous tissue. These appearances suggest a perithelioma originating in the small blood-vessels.

DISCUSSION.

Mr. HERBERT TILLEY suggested making a small incision in the growth and burying a strong radium tube within it.

Dr. WYLIE thought it would be difficult to treat this growth by any surgical measures, and he suggested that the exhibitor should treat his patient by exposing her to erysipelas. He had a case of epithelioma, in which the disease was extensive, and the patient was considered hopeless. Erysipelas broke out in the ward and the patient contracted it and was very ill for several weeks. On recovery the epithelioma was cured and there had been no recurrence. The treatment was drastic and dangerous, but the disease was dangerous, too. Dr. Wylie was so struck with the result that he had been trying Coley's fluid once more in such cases.

Dr. DUNDAS GRANT said that a material point in this connexion was as to whether the tumour in Dr. Wylie's case was identical in nature with the one under discussion in Mr. Whale's. The one, he thought, was an endothelioma and the other an epithelioma.

Mr. STUART-LOW said that he had had a similar case which had apparently got well, but after an attack of erysipelas the malignant disease returned in six months time in an aggravated form, and the patient died.

Mr. W. D. HARMER said that the difficulty in these cases was to know exactly the nature of the growth, whether it was sarcoma or endothelioma. The latter type of growth would sometimes disappear in three weeks after a large dose of buried radium, but it was not yet known whether permanent cures could be obtained by this means. He believed that the earliest case of endothelioma reported as cured by radium only dated back three or four years, and that in some of these cases it would be found that the cure was only a temporary one.

Sir WILLIAM MILLIGAN urged that if radium was to be used it should be used in massive doses and with needles—at least a dozen needles—so as to get the benefit of very intense cross-firing.

Mr. WHALE said that he was very much obliged for the suggestions. He did not, however, feel that he would like to give the patient erysipelas. He had been thinking of trying diathermy, and two members on seeing the case had strongly recommended surgical operation. The case differed in nature from Dr. Wylie's, which was, he understood, an epithelioma.

Dr. DAN MCKENZIE said that on seeing the case he had urged that it be dealt with surgically, and he still maintained that opinion. He would certainly be inclined to attack that growth by a Moure's incision and free exposure, and thought it could be treated successfully.

Patient (Gunner S.) from whom the Piece of Tin shown at the last Meeting was removed by Posterior Œsophagotomy.[1]

By H. L. WHALE, F.R.C.S.

THE scar is linear and so far back as to be invisible when the patient is clothed and wears a high collar. This is one advantage of approaching the œsophagus from behind the sternomastoid.

Two Cases of Œsophageal Stricture in Children.

By C. W. M. HOPE, F.R.C.S.

Case I.—Male, aged 7½, was admitted to King's College Hospital, with the history of having vomited at least once a day since about the age of 2 months. An X-ray photograph shows a well-defined stricture of the œsophagus behind the pericardium. At Dr. Frew's request I examined the child with the œsophagoscope on February 17, 1915, and found enormous dilatation of the upper œsophagus. The dilated portion contained much undigested food; the lining membrane above the stricture was very unhealthy, in parts denuded of epithelium, and bled easily on swabbing, and towards the right side were seen some very definite areas of scar tissue. The mouth of the stricture was well defined, and admitted with difficulty a bougie of 5 mm. diameter. It

[1] *Proceedings*, p. 70.

was dilated up to a bougie of 10-mm. diameter. To date of reporting (February 26) the boy has only vomited once, and has gained in weight about a pound a day. Up to now he has never had solid food, but has lived purely on liquid or sloppy food.

Case II.—Baby, aged 18 months, was admitted to King's College Hospital on February 17, 1915, with history of daily vomiting since birth. Not emaciated. On examination the same day with the œsophagoscope, the whole of the thoracic œsophagus was found greatly dilated, but no food lying in the dilated area. The mucous membrane of the sub-diaphragmatic portion was found thrown into large transverse folds, but bougies, as well as the second largest Killian tube, could be passed into the stomach without any difficulty. There has been no more vomiting to date of report (February 26), and child can now eat bread and butter. This case is apparently of the same type as that of the congenital hypertrophic stenosis of the pylorus, which does so well with simple dilatation with Hegar's dilators through a gastrotomy opening. Skiagram shown, taken three days after dilatation.

Mr. HOPE said that since writing his notes he had obtained more history relating to the condition of the elder boy (Case I). The child had had solid food given to him, but this never reached his stomach. Sometimes he would keep a whole meal in the dilated œsophagus or sometimes only a portion, and then vomit it up, this occurring eight, ten, or twelve times a day. There was no history of any corrosive having been swallowed. About four years ago he (the speaker) saw at Newcastle a case under Mr. Martin's care in which there was a very tight fibrous stricture in a child aged 4, the stricture coming on directly after an attack of diphtheria. There was very marked scarring in the dilated upper part of the œsophagus, and one could not get even the finest filiform urethral bougie through the stricture. A gastrostomy had already been performed when the case was seen, and the child eventually died. Mr. Hope also said that the children in neither of his cases were emaciated, although they were never properly fed. Even the small child was quite well covered. With regard to the condition of the stricture in the small child, there were very marked rugæ seen at the lower end of the œsophagus, and dilatation certainly as big as was seen above the malignant stricture in any grown-up patient. Dr. Still rather thought that there was a predisposing condition of spasm. He knew that the existence of spasm was doubted, but Dr. Still appeared to think that there was such a thing in the case of the œsophagus, similar to the condition occurring at the pylorus in young children.

Œsophagus from a Child, aged 2½, showing Escharotic Stenosis.

By Dan McKenzie, M.D.

History: In May, 1914, the patient swallowed half a teacupful of a poison for bugs. The mother induced vomiting by tickling the pharynx with her finger, and half the quantity was regurgitated. After swallowing the poison the tongue was protruded and swollen and the lips were bluish. Great limpness followed the swallowing of the liquid. After that he was unable to swallow solid food and sometimes not even liquids, the food returning before it entered the stomach.

He came under my care on November 20, 1914, quite unable to swallow solids and semisolids, and only getting down small quantities of liquids. The X-ray examination showed complete impaction in the œsophagus, at the level of the sterno-clavicular articulation. Seven or eight attempts to dilate the stricture were made. All failed. At first I succeeded in passing fine tubes and bougies, but the immediate effect of these efforts upon the strictures was discouraging. Feeding was mainly carried on *per rectum*. After a distressing illness the patient died on February 1, 1915.

Pathological report by Dr. Wyatt Wingrave.—Œsophageal stricture: The specimen was removed entire, from tongue to duodenum inclusive. About 3 in. from the cricoid level stricture commences abruptly and gradually diminishes towards stomach, involving about 1½ in. It consists of apparently dense cicatricial tissue involving both mucous membrane and muscle, with some extension to cellular tissue around. The lumen is reduced to about 1 mm., only admitting a probe. Above the stricture the tube is dilated, and its mucous membrane is thrown into well-marked transverse rugæ, but not markedly reduced in thickness. Below it is not dilated, and the cicatricial tissue *gradually* ceases, not abruptly, as at the upper boundary. The stomach is very small and walls very thin, altogether smaller than a normal stomach at that age. Above and below the stricture the mucous membrane is normal, and there are no signs of present or past erosion in any part of the tract above or below stricture. The aorta is normal and there is no arteria aberrans.

Dr. DAN MCKENZIE said that Dr. Wyatt Wingrave felt very strongly that the stricture in this case was more likely congenital stricture of the œsophagus than due to corrosive fluid. He (the speaker) did not agree with him, but in justice to Dr. Wingrave and his opinion, he thought it only right that it should be stated.

Epithelioma of Pharynx, Tongue, and Cervical Glands in Male.

By W. STUART-LOW, F.R.C.S.

SINCE the last meeting, when this case was shown,[1] the tongue, pharynx, and glands have, on three occasions, been treated by diathermy. Free incisions having been made over the enlarged cervical glands, and the tissue reflected, punctures were made in the stony-hard glands with beneficial effect, the enlargements being greatly reduced and the pain much diminished.

Rabbit's Vertebra in the Right Lower Bronchus removed by Superior Bronchoscopy.

Shown by J. DUNDAS GRANT, M.D.

BONE swallowed twenty weeks before admission to Brompton Hospital under Dr. Batty Shaw. Physical signs indicated bronchiectasis in the right lower lobe. Expectoration copious, extremely fœtid, sometimes tinged like anchovy sauce, and 9 to 10 oz. daily in amount. Radioscopy by Dr. Stanley Melville confirmed these signs, and showed further a discrete dense opacity at the level of the fourth interspace anteriorly and the eighth rib posteriorly, probably foreign body.

Dr. Grant performed bronchoscopy and removed a rabbit's vertebra from right inferior bronchus at a distance of about 12½ in. from the teeth on February 18. The patient refused bronchoscopy under local anæsthesia (having previously been submitted to it elsewhere) and was, therefore, anæsthetised in dorsal position. The field of operation in the trachea and right bronchus was swamped with fœtid, blood-stained muco-pus. The bone was removed by means of Killian's toothed forceps, and the bronchoscope had to be withdrawn along with it.

[1] *Proceedings*, p. 66.

There was no subsequent laryngitis and the symptoms have nearly disappeared, the sputum now averaging 4 oz. daily, and later ½ dr.

Dr. Grant said that at the time of the extraction he gave instructions for tracheotomy instruments to be kept at hand, but no need for them arose.

Foreign body, skiagram, and patient exhibited.

Case of Removal of Epithelioma of Larynx.

By W. M. Mollison, M.C.

F. B., aged 45, complained of hoarseness of about four months' duration. He was seen in November, 1914, at Guy's Hospital; there was then a sessile swelling about the left cord. Laryngo-fissure was performed. The whole left cord was removed, together with the tip of the vocal process of the arytænoid; the growth involved also the anterior quarter of the right cord, and the anterior half of this cord was also removed. The specimen is shown. Recovery was rapid. Two months later a distinct swelling was seen at the anterior commissure; this swelling is still present, but much smaller. However, with suspension laryngoscopy, a small piece was removed which proved to be fibrous tissue only.

DISCUSSION.

Dr. Dundas Grant said that in such cases an apparent re-growth about the anterior commissure sometimes caused anxiety, but it was usually simply granulation tissue. It was thought to be more apt to take place when a suture was used, and it ultimately disappeared.

Sir StClair Thomson said that these appearances of re-growth always gave rise to anxiety during the first few months of convalescence. One learned by experience how to distinguish them. He thought that the fresh growth in this case was nothing more than a simple granuloma. These granulomas were much more pedunculated or semi-pedunculated than the original growth. He pointed out that in this case, in addition to the fresh granuloma, there was a little red fleshy pimple, such as one saw over a piece of exposed cartilage.

Sir Felix Semon said that his own experience was that when apparent recurrence took place, one should not directly proceed to a second large operation. In the great majority of cases the apparent recurrence was only granuloma. He wished to lay emphasis upon this point. In a case of his, in which he had

performed laryngotomy, a re-growth was discovered in the anterior commissure while he himself was away on holiday. Although he had suggested the possibility of simple granuloma, the family doctor had a second thyrotomy done, and, on pathological examination, simple granuloma was detected. After six weeks the patient came to him again for a fresh outgrowth in the anterior commissure, and he then followed his own principles, and removed it with forceps from within. It was a simple granuloma, and after its removal in this manner the patient remained perfectly well. He strongly advised any one who had a case of the kind not to perform at once a second big operation, but first to remove a piece of the growth for examination from within.

Case of Fibroma in Nasopharynx.

By J. L. Irwin Moore, M.B.

Boy, aged 16, with growth—fibroma—in nasopharynx, originating apparently from left half of sphenoid, and occupying the greater half of post-nasal space. The patient complained of difficulty in breathing at night and on eating food. There had been no spontaneous hæmorrhage. Opinions were invited as to best method of removal.

DISCUSSION.

Dr. Dundas Grant thought this was a small-based fibroma, and its removal might be effected without any great anxiety. It could even be done, he thought, with the old-fashioned form of adenoid forceps with large spoon blades.

Mr. Herbert Tilley said he would certainly oppose Dr. Grant's method of treatment most strongly. In the first place, on careful examination this growth appeared to shelve up the posterior wall of the nasopharynx—i.e., he believed the growth was not pedunculated, but that it was a sessile fibroma, and probably very vascular. He would be sorry to tackle that with "an old pair of adenoid forceps," or without a definite plan of campaign. It might be possible to remove it through the nasal passage. One authority had stated that he had never seen a nasal fibroma which could not have been removed through the nasal passages. But, assuming that it could not be removed in that way, this particular fibroma having a large base of origin, he would suggest making a Moure's incision (lateral rhinotomy), and treating the growth through the large opening thus made. It would then be an excellent case for diathermy. The growth was very vascular, and one ought to regard its removal as a serious operation. His suggestion, if acted on, would involve an external scar, but the patient was a very young boy, and if the operation was successful the scar would be unnoticeable in a few years' time.

Sir StClair Thomson said that Mr. Tilley had just spoken of the "nasal passages"; he took it that he included the mouth. All these extensions, although they might have secondary adhesions in the nose, in the sphenoid, and the antrum, could be extracted through the mouth. On this matter he spoke without any personal bias. He had not worked on such cases, but he had a great opportunity of seeing some of them in Paris, and Mr. E. D. Davis showed a very similar case at the last meeting of the Section—a case which was described as a fibroma of the nasopharynx, a thing pathologically innocent, but clinically malignant. Mr. Davis removed it through the mouth. He (the speaker) thought it a great pity to disfigure anybody by a scar through the nose. In Paris they neither split the soft palate nor operated through the face. They placed their patient in a Trendelenburg position, and had their curettes working sideways, one cutting surface working one way and the other the other way, and passing up very quickly into the nasopharynx. They had forceps—not old adenoid forceps—ready to seize and wrench this growth out. Their great principle was to operate very quickly, otherwise the blood poured out. He had seen Georges Laurens, a leading laryngological operator of Paris, get it out so quickly that a sponge put into the nasopharynx was all that was required.

Dr. Dundas Grant said there was no such epistaxis as would be present if the growth was very vascular, and no deafness such as was usual when any infiltrating malignant growth occupied this region and extended to the Eustachian tube. He had not had the advantage of following up the result of microscopical examination or of digital palpation.

The Chairman said if the case was of the nature it was believed to be—namely, a nasopharyngeal fibroma or angeio-fibroma, then he would hesitate before attacking it with only adenoid forceps; although the principle of the adenoid forceps was one which might be developed and applied.

Dr. Irwin Moore explained that the patient came up to the hospital to be operated on for adenoids. After examining with the post-nasal mirror and feeling carefully with his finger, he came to the conclusion that the growth was a fibroma, on account of its extreme hardness. He was fortunate in coming across a case at such an early stage before extensive growth had occurred. Many years ago, when he first started practice, he had a patient with a similar growth, but so extensive that it hung down into the throat and the lower end could not be seen without depressing the tongue. He removed it through the mouth, using a hot platinum wire passed through the nose. He still had the specimen. The growth recurred, and the patient afterwards attended the Leicester Infirmary under Dr. Bennett. He once saw Sir Watson Cheyne operate on a similar growth which he believed was about the same size as the one exhibited. Having first done a preliminary laryngotomy, he split the soft palate and removed the growth by means of a periosteal elevator—the head hanging well over the end of the table. In reply to a question

by Sir Felix Semon, the speaker said that, so far as his recollection went, there was not very much hæmorrhage. In his own case he was inclined to wait a little and see whether the growth grew rapidly. He thought there would not be much difficulty in removing it through the mouth.

Case for Diagnosis.

By Cyril Horsford, F.R.C.S.

Patient, a male, aged 60. Slight hoarseness since November, following on wetting of feet. His voice has never been clear since. His work does not entail much use of the voice.

DISCUSSION.

Mr. Clayton Fox thought that this case would prove to be one of epithelioma. There was considerable infiltration of the left cord and slight impairment in its mobility. Of course the Wassermann test should be taken, and iodide of potassium with mercury administered. If no improvement followed, a laryngo-fissure ought to be performed and the cord and adjacent tissue freely removed.

Mr. Stuart-Low said that he considered that the left vocal cord showed indications pointing to malignant disease.

Mr. Herbert Tilley thought that there was no doubt that the laryngeal growth was malignant. He pointed out that the anterior part of the *left* cord was quite white and the posterior red.

Sir Felix Semon thought it a beautiful case for laryngeal fissure. Without doubt it was malignant. The sooner the operation was performed the better would be the chance for the voice afterwards. He agreed that a Wassermann's test should be taken.

Sir StClair Thomson said that it was doubtless clinically malignant. He raised the question as to whether the white on the opposite cord was white froth or was due to a little thickening there.

[*Postscript.*—Wassermann reaction negative. No improvement with potassium iodide—slow but steady increase of growth up to this date, April 20. Patient has refused operation.—C. Horsford.]

Laryngological Section.

May 7, 1915.

Dr. WILLIAM HILL, President of the Section, in the Chair.

PART I.—CIVIL CASES.

Malignant Tumour at Base of Tongue almost dispersed by Radium.

By WILLIAM HILL, M.D.

AUSTRIAN subject (registered), aged 55; growth, size of large walnut, ulcerating posteriorly and with secondary involvement beneath the left angle of jaw; left side of tongue fixed; occasional hæmorrhage; salivation; dysphagia; constant pain; insomnia. Insertion in two places of small, hollow, needle-like emanation apparatus of platinum 0·3 mm. thick, containing an initial charge equivalent to 40 mg. of RaBr. for twenty hours altogether. All symptoms have disappeared, and the tumour is so reduced that it might be easily overlooked on superficial examination. The secondary involvement was dealt with before the date of meeting by an external application of screened radium salt. A water-colour drawing made by the patient, an artist by profession, of the original appearance with the mouth open was exhibited.

Case for Diagnosis.

By W. M. MOLLISON, M.C.

G. B., AGED 72, was sent to Guy's Hospital in November, 1914; he then had the appearance on the anterior pillars of fauces that he has now. The Wassermann reaction is negative, and, in spite of administration of mercury and potassium iodide, no change has taken place. The patient complains of pain on swallowing, severe at times. Suggestions as to diagnosis and treatment will be welcomed.

Case of Bent Nose Straightened by Submucous Rotation of the Cartilaginous Septum.

By J. Dundas Grant, M.D.

The nose pointed so far to the left as to be extremely conspicuous. The convexity of the deflection of the septum caused considerable obstruction of the right nasal passage. The nasal bones proper and the osseous septum were comparatively symmetrical.

The incision was made as for the submucous resection, but very close to the tip and on the side opposite to that to which the nose pointed. The cartilage was very carefully cleared, and was incised well forward. Killian's original plough was cautiously used so as to cut as much as possible of the curved cartilage, including the lower corner which pointed to the left. The wedge-shaped portion was cut as close as possible to the perpendicular plate and to the vomer, but not to the farthest extent, backwards, so as to leave a slight attachment posteriorly. It was then seized with flat forceps and rotated on its antero-posterior axis through half a circle in the direction contrary to the hands of a watch, so that the upper edge was to the right, and then downward to "six o'clock," where it was for the moment a little over-twisted. The part that originally pointed to the left now pointed to the right, and the lowest part was now uppermost. Any "spring," therefore, tended to incline the cartilage to the right side. The incision was carefully sewn up, and well-vaselined plugs of cotton-wool were inserted.

The plugs were retained for two days and were then removed. The incision was uniting, and there was no tendency for the cartilage to push it open. The septum was then in a good position, and it appears to have healed well and adhered in its new position. The soft parts had still a slight cant to the left, being somewhat redundant on the right side and somewhat diminished on the left. The appearance of the nose was, however, so satisfactory that surgical interference was not further called for, and the lady did not feel inclined to be troubled even with the pads of a nose-machine. She applied strapping to fix the nose to the right side for a time.

N.B.—Dr. Grant thinks that, if he had a similar case to deal with again, he would make the incision on the side towards which the nose pointed, so that the spring of the cartilage should not exercise pressure on the sutured incision.

A Nasopharyngeal Fibroma removed through the Mouth without any Preliminary Operation.

By Irwin Moore, M.B.

This patient, a boy, aged 16, one of triplets, was shown at the last meeting of this Section on March 5. The growth was in an early stage and apparently similar to that exhibited by Mr. E. D. Davis on February 5 last. The chief symptoms were that six months ago he had to give up singing in a choir on account of difficulty in breathing, and choking attacks at night. Occasionally streaks of blood were coughed up, but there had been no spontaneous hæmorrhage. Marked heaviness and sleepiness, with great difficulty in getting him up in the mornings. He was supposed to be suffering from adenoids.

The growth and its attachments could not be seen from the anterior nares on account of a marked deviation of the septum with hypertrophic rhinitis. Resection of the septum was therefore performed a week previously. In removing the growth no preliminary operation of laryngotomy was performed, and the division of the palate was avoided. The anæsthetic was first given through Kuhn's oro-tracheal intubation apparatus, but it was found unnecessary and in the way. The hanging-head position, together with the free nasal channels, proved all that was necessary to keep the trachea free from blood. The soft palate was tied out of the way by a piece of soft rubber tubing passed through the nose and mouth. The growth was attacked and removed through the mouth. It was found to be sessile and attached by a broad fibrous base to the basi-sphenoid and occipital bones, with a prolongation extending and firmly attached to the spheno-ethmoidal recess of the left naris. Great difficulty was experienced in detaching this root—nor was it possible to grasp and remove the growth by means of the usual post-nasal forceps, until the root was fully detached. The manner in which the growth avoided the grasp of the forceps was very remarkable; its growth was very vascular, blood pouring out of the mouth and nose to an alarming extent during the later stages of the operation, but it ceased on detachment of the nasal prolongation.

The specimen was an irregularly rounded mass, of a red colour from extravasated blood, and of a hard consistency. It measured $1\frac{1}{2}$ in. by $1\frac{1}{4}$ in. in its longest diameter and 1 in. in its shortest.

Report by Professor Shattock.—Microscopic examination shows the neoplasm to consist of coarse, intersecting fibres of hyaline connective tissue. Where the swelling of the fibres is most pronounced, the corpuscles have in cross-section a stellate figure, the processes of the cells occupying the fissures between the swollen fibres. In some areas the capillaries are so numerous and so dilated that the growth might be described as "cavernous" and classed as angiofibromatous.

The patient is shown, together with the growth, also a microscopic section and photomicrographs showing various portions of its structure.

Growth in Maxillary Sinus extending into Nasopharynx.

By Irwin Moore, M.B.

E. A., male, aged 42, attended the London Throat Hospital on March 5, under Dr. Cathcart, who very kindly transferred the case to me. He complained of difficulty in breathing through the nose during the past twelve months, with considerable amount of watery discharge sometimes mixed with pus, occasionally slightly bloody on blowing nose. There is a history of syphilis twenty-five years ago.

On examination of the right nares, the middle meatus was found to be occluded by a growth, below which there was free communication with the nasopharynx. A considerable amount of pus was seen in right fossa issuing from under the middle turbinal. The right side of the nasopharynx was occupied by an irregular œdematous-looking growth, nearly filling that cavity. A brawny swelling over the outer wall of the sinus involved a portion of the soft parts. The right maxillary sinus, on transillumination, was found to be very dark, and on being punctured the needle could be felt entering growth, and attempts to wash out failed. There was no bleeding on withdrawal of the needle. The swelling over the cheek increased considerably during the three following days.

Skiagrams by Dr. Finzi show both antra somewhat opaque, but the right more so than the left, especially in the oblique views. The right sphenoidal sinus is very opaque, and also the ethmoid cells on the right side; these data being obtained by a comparison of the lateral with the anterior views.

On April 20 an opening was made through the canine fossa, and

the antrum was found to be occupied by a growth extending across and filling the upper two-thirds of the cavity, from the intranasal to the outer wall, which was necrosed, and infiltrating the cheek. Pieces removed from the growth for microscopical examination were of a soft consistence. There was little bleeding. A radium tube of 85 mg. strength and 1 mm. silver screening was inserted well into the growth and left in situ for twenty-four hours. There was very slight reaction and no rise of temperature.

Report by Dr. Eastes on section of growth.—"This specimen is composed of loose connective tissue, not very rich in nuclei. It is somewhat œdematous, and is also infiltrated with small round cells, a fair proportion of which are polymorphonuclear leucocytes. Another feature is the presence of several capillaries which have thickened and hyaline walls; in my opinion the tissue is not neoplastic, but of a chronic inflammatory nature.

The report on the Wassermann reaction is negative.

Opinions are invited as to diagnosis and treatment.

Section of the growth is shown, also skiagrams by Dr. Finzi.

Further Notes on a Case of a Gentleman, aged 59, with Malignant Stricture of the Œsophagus.[1]

By Irwin Moore, M.B.

The patient formerly weighed 17 st. 7 lb. When first seen on September 18 he weighed 14 st. 12 lb., having lost 2 st. 9 lb. When shown before this Section on November 6 the tube through which he had been able to take 9 pints of food daily had been in position for seven weeks, and the patient had gained 6 lb. The tube was removed on that date, but required re-insertion nine days later on account of temporary obstruction. During this period patient was able to swallow milk, beef-tea, and bread and milk, with very little difficulty. The original tube has now been in situ nearly six months.

The patient writes a cheerful letter, and reports as follows: "The tube is giving every satisfaction. I am still taking about 8 pints of food daily, consisting of beef-tea, junkets, light-boiled eggs, custard, Benger's

[1] Shown at the Section meeting on November 6 last, to illustrate the benefits of Dr. William Hill's oro-œsophageal feeding tube.

food. I look well, but I feel a little weak at times, and cannot stand much fatigue."

The X-ray photograph by Dr. Finzi of the stricture is shown, together with one showing the feeding tube in position.

Patient suffering from Recurrent Columnar-celled Carcinoma of the Antrum.

By W. Stuart-Low, F.R.C.S.

A man who has been shown previously on two occasions after operations two years ago on the left maxillary antrum for the removal of a columnar-celled epithelioma. There was a recurrence six months after the first operation, and a very thorough second operation was performed eighteen months ago. The growth has now recurred in the ethmoidal region, and the cavity of the orbit has become invaded. Diathermic puncture has been applied with good results in diminishing the size of the mass in the nasal cavity.

Microscopic specimen and pathological report by Dr. Wyatt Wingrave.—"The growth is of the columnar or 'palisade' cell type, merging into the alveolar, most of the cells resembling the normal surface epithelium of the region, but devoid of cilia. They are arranged on a fimbriated and branching framework of fibrovascular tissue which forms the stroma. It therefore consists of epi- and meso-blastic elements suggestive of adenoma. In some parts the cells only form a single layer, but for the most part they are closely packed, the villous type changing to the alveolar and original features lost. Many of the nuclei exhibit hetero-mitotic division. The tumour therefore presents a striking resemblance to malignant polypus, or tubular epithelioma of the lower bowel, and to villous growths of the bladder and uterus. When differentially stained by muci-carmine or Pappenheim, heteroplastic characters are well shown. While normal and adenomatous tissues show mucigen granules and 'chalice cells,' their neoplasm shows none. Structure is therefore imitated, but not function. This type of neoplasm is by no means rare in the nose, for it has constituted 40 per cent. of malignant cases in that region, but it is very rare as a primary growth in the pharynx and larynx. In the mouth it has not been recorded. It is usually associated with, or may start from, the region of the ostium maxillare. Growth is rapid, consistence is very soft and brittle, it bleeds

freely, and although locally malignant is but rarely found as a secondary deposit in glands. The true significance of the tumour is difficult to establish when a small fragment only is supplied for examination, therefore an adequate mass is usually necessary for diagnostic purposes."

Sections: (1) Showing neoplasm; (2) chalice cells in normal mucous membrane.

New Growth in Post-nasal Space.

By Frederick Spicer, M.D.

E. G., female, aged 70, married. Patient was brought to the Metropolitan Ear, Nose, and Throat Hospital on February 9 last, complaining of inability to breathe through the nose, deafness, and rapid wasting. She was very feeble, had to be assisted in walking, and was breathing through open mouth, from which saliva was dribbling. A quantity of fœtid muco-pus was being continually discharged from the nostrils. She appeared seriously ill.

On examination, the nasal fossæ were found to be blocked with prolongations of a growth situated in the post-nasal space. The growth was easily seen from the mouth by lifting up the soft palate. It was greyish in colour, and the surface rough and cauliflower-like in appearance, growing from the vault, not particularly hard, and bleeding on examination.

Under simple cleansing treatment a great improvement in her condition took place and the size of the tumour diminished.

Pathological Report (Dr. Leathem). — "Section shows lymphoid tissue covered with squamous epithelium."

Postscript.—A fortnight after the exhibition of this patient her strength improved sufficiently to admit her submitting to an operation. Under cocaine, the tumour was completely removed by cutting forceps; there was very little hæmorrhage, and the patient made a quick recovery. The growth was submitted to my colleague, Dr. Hemington Pegler, for examination, who pronounced it to be a "lymphosarcoma." The specimen was presented to the Museum of the Royal College of Surgeons.

Halfpenny embedded in Œsophagus.

By W. H. Jewell, M.D.

Child, aged 22 months. There was a history of its having swallowed a coin ten days before admission to hospital. Death occurred on the thirteenth day after the accident, with perforations of the right and left sides of the gullet. There was occasional sickness after food during the first five days, but afterwards there was no return of food. No dyspnœa. The œsophagoscope was passed with the aid of the fluorescent screen, and the coin located just below the sterno-clavicular articulation embedded in the posterior wall of the œsophagus. The circumference of the coin and four-fifths of its anterior surface was covered with mucous membrane, making it impossible to apply forceps or hook or to dislocate it from its bed.

Specimen shown with coin in situ.

Pharyngeal Fistula in an Old Man after Operation on Abscess in the Neck.

By W. Douglas Harmer, M.C.

Male, aged 80, admitted to St. Bartholomew's Hospital under the care of Mr. H. W. Wilson. On March 17 operation for the removal of the prostate. This was well tolerated, and the patient made good progress until April 10, when the left side of the neck suddenly became inflamed and a swelling appeared, apparently connected with a gland. Four days later a definite abscess had formed and was incised. On April 24 paralysis of the soft palate, with regurgitation of fluid through the nose, was first noticed. Soon afterwards it was found that, when taking his meals, food passed from the mouth through the wound in the neck, and as the abscess was not draining well, the opening in the neck was enlarged on April 28. There are now two clean-cut openings in the pharynx: (1) in the middle of the left tonsil; (2) near its upper pole. The wound in the neck and the suprapubic incision are both healing very sluggishly.

PART II.

WARFARE INJURIES AND NEUROSES.

THE PRESIDENT, having ascertained from the meeting that it was the wish to devote the time at their disposal to the discussion of injuries to nose, sinuses, and throat incidental to war, suggested the grouping of subjects and cases for discussion as follows: (1) Introductory paper by Majors Milligan and Westmacott; (2) functional cases; (3) injuries of the upper face, involving the palate, nose, and sinuses, and injuries of the lower face, involving the mouth, fauces, mandibular and submandibular regions; (4) injuries of the neck involving the lower pharynx, larynx, trachea, and gullet.

Introductory Paper by Major Sir WILLIAM MILLIGAN, M.D., and Major F. H. WESTMACOTT, F.R.C.S.

THE Second Western General Military Hospital, whose headquarters are situated in Whitworth Street, Manchester, was opened for the reception of wounded soldiers upon September 20, 1914. Since that date over 14,390 cases of sick, maimed, or disabled soldiers have passed through its portals from the Expeditionary Forces, many having received such serious injury from shot or shell as to permanently incapacitate them from ever following their occupations again. Of these 14,390 and odd cases the greater number have suffered from injuries necessitating their coming under the care of the general surgeon. A very fair sprinkling, however, having received serious damage to the pharynx, larynx, nose, or ear, have been placed under the immediate charge of my colleague Major Westmacott and myself either at Whitworth Street, the large military wards in the Manchester Royal Infirmary, or at one of our principal annexes in High Street, Oxford Street.

Our experience has been limited to the treatment of injuries to the special organs we are here most interested in, the War Office having from the commencement very wisely determined to utilise the services of those in charge of the special departments at the Royal Infirmary for the work with which they were most familiar.

At the commencement of the war when our troops for strategical and other military reasons were retreating, it was noticeable that the larger percentage of those who were injured were wounded below

the belt, as it were—the lower abdomen, the pelvis, the legs, and the feet. Later on, when the Army entrenched itself in the North of France and in Flanders, injuries to the head, neck, and upper part of the thorax became more frequent, the actual conditions of fighting explaining the difference in the situation of the wound or wounds received. As might be expected, and as unfortunately proved only too true, in many instances the wounds received proved immediately fatal or fatal within a few hours or days. Many of the injured whom we have seen have had miraculous escapes from death, the projectile or piece of shrapnel passing within a hair's-breadth as it were of some important structure, which if lacerated would have meant practically instantaneous death.

What has struck us very forcibly has been the comparative immunity from septic complications of many of the injuries of the face and neck, and this we attribute, in part at any rate, to the absence of clothing in these particular regions and to the consequent non-contamination of the wound with portions of uniform, soiled underwear, earth, manure, &c., and also to the fact that in many cases the tracts of infection have communicated with the external air by way of the nasal passages, the mouth, the larynx, &c., a circumstance unfavourable to anaerobic infection of discharges from the wound.

In many of the cases seen by us the velocity of the projectile fired at a comparatively short range has been such as to pass clean through the head or face, the wound of exit being hardly if any larger than the wound of entrance. The fact also that the bones of the face and of the framework of the nose are thinner and more brittle than those of the skull and extremities has probably had much to do with permitting the projectile to pass more cleanly through than is the case in, say, the long bones of the arms or legs, where splintering and severe comminution have been so frequent and have proved so disastrous. The problems, therefore, we have had to face have from the surgical point of view been considerably easier than have those pertaining to wounds of the extremities or internal organs, where wound infection has been practically invariable.

The principle of securing and of maintaining free drainage and of utilising to the full the antibacterial properties of the individual's tissue fluids has been invariably acted upon. Where a projectile or piece of shrapnel has become deeply embedded in the bony framework of the face, nose, or that portion of the vertebral column corresponding to the epipharynx, the pharynx, or the hypo-pharynx, when its

position has been accurately located by radiography and when there is neither troublesome hæmorrhage nor evidence of sepsis, our practice has been to leave it severely alone, repeated clinical and radioscopic examinations being made to ascertain if it remains quiescent. When, on the other hand, the projectile is quite superficial and accessible, and when its removal would not in any way interfere with the integrity of any important structure, our practice has been to remove it by the easiest possible route, with due observance of ordinary surgical principles.

We are satisfied that in many cases the *noli me tangere* attitude is not only in the best interests of the patient, but is also the most scientific. All meddlesome surgery, more especially by those untrained and unaccustomed to surgical manipulations, is in the interests of the wounded to be highly deprecated. We are strongly in favour, in the surgical treatment of warfare injuries and wound infections, of standardising the methods of treatment so as to avoid undue experimentation, and of the following out of individual and often insufficiently tried methods of treatment, the actual results of which the surgeon is often little aware of. At the Second Western General Hospital there has been a remarkable uniformity of treatment, alike conservative and scientific, with the result that amputations have been very few, and many a limb which on admission looked most unpromising has been saved to its rightful owner. When severe laceration has taken place and tracts of highly infected tissue are present the underlying principle in treatment has been to open up the wound in every direction, to irrigate with antiseptic lotions, especially hydrogen peroxide, and to encourage a free flow of lymph by lavage, baths, &c.

In injuries to the nose and nasopharynx the immediate anxiety has often been the arrest of hæmorrhage, and the remote one how best by some form of plastic operation to restore function and appearance. In several of our cases hæmorrhage has been severe and it has not always been possible to ascertain its actual source. In but few cases, however, has it been necessary to do more than plug from the front. In one case, where a bullet entered the side of the face just below the left malar bone and passed across the nasal passages, emerging just under the malar bone upon the opposite side, the turbinal mucosa was much lacerated. Several attacks of severe hæmorrhage having taken place prior to admission to hospital, the nasal passages were carefully cleansed, a few fragments of necrosed bone removed, and large doses of ernutin ordered, with satisfactory results. In a few

instances the bullet has been spat out after reaching the mouth, and in one case we removed a German bullet which was normal in size and shape, from between the layers of the soft palate. In another case, that of a Belgian soldier, a bullet passed through the left nasal process of the superior maxilla and became embedded in the posterior antral wall upon the left side. Two attacks of severe bleeding followed. Some necrosed bone was subsequently removed after cutting through dense adhesions between the septum and inferior turbinal body, but the bullet was not interfered with and is there to this day, as may be seen by reference to the X-ray photograph in the adjoining room. He recovered completely.

In comminuted injuries of the framework of the nose the maintenance of a free passage and the prevention of adhesions has presented considerable difficulties. As an effective splint we have found nothing better than the finger of a disused rubber glove packed with gauze. It is light, unirritating, and capable of being made practically any size according to the amount of packing introduced into it. When the bridge of the nose has been severely injured and depressed, with the result that its tip assumes the *en lorgnon* position, a most unsightly spectacle, some form of plastic operation is required. So far we have not been called upon to perform any such operation, although two cases are shortly to be dealt with.

Injuries to the larynx have been comparatively rare. In one case a bullet struck the side of the neck at the level of the upper border of the thyroid cartilage upon the left side, was deflected by the cartilage, ran down the neck and became embedded behind the left sterno-clavicular joint. Severe perichondritis of the arytænoid cartilage resulted, accompanied by such an amount of œdema as to necessitate a tracheotomy. When last seen the patient was still wearing the tracheotomy tube and the œdema was slowly subsiding. Quite a number of cases of severe laryngeal catarrh from the trenches have been seen, but have presented no features demanding special attention.

Another class of injury quite different from the type of projectile injury we have been considering, but none the less interesting, is injury to motor or sensory nerve tracts coming under the heading of "warfare neuroses." We understand that neuroses of the organ of hearing are not included in our discussion to-day, and this we consider unfortunate, as warfare neuroses of the eighth nerve, associated with such other neuroses as loss of smell, loss of speech, and even loss of memory, have been by no means infrequent, and open up a field of inquiry both

pathological and psychological, at once interesting and instructive. When there has been a definite objective injury to any one particular nerve or group of nerves, preventing the passage of motor or sensory impulses to or from the central nervous system, the resulting lesion or lesions are readily explicable, but where there has been no local lesion, no outward mark of internal disturbance, the problems offered for our solution are extremely difficult.

As an example of injury with definite objective evidence we may cite the case of a soldier suffering from left recurrent paralysis from a bullet wound in the face. In this case the bullet is embedded in the body of the second cervical vertebra. In its passage it must have injured those fibres of the vagus which go to form the "nervus recurrens" at a distance of from 1 to 2 in. below the base of the skull. Or the case of another soldier seriously injured by a bullet which passed from just below and behind the lobule of the left ear, traversed the skull and so completely disorganised the contents of the right orbit as to necessitate removal of the eye. In its passage it so damaged the cribriform area as to produce almost complete anosmia.

Nerve injuries such as those present no particular difficulties, but when one comes to cases of loss of sight, of speech, hearing, smell, or memory, without any objective evidence of damage done, we are at once faced by problems of real moment. We speak constantly of concussion as the cause of such cases, but what is concussion? Is concussion always attended by definite organic changes, however minute, or are there certain molecular alterations in tissue fluids or tissue cells which so modify the transit of impulses from the higher nerve centres as to produce at times temporary, at times permanent, loss of function? Again, in certain of these cases is there an element of hysteria, or is it a subconscious manifestation of the effects of sheer fright? What has struck us very forcibly has been the rapidity with which many of these cases recover when the patient is nursed under favourable and quiet surroundings, "far from the madding crowd's ignoble strife."

If we may be allowed to revert for one moment to the auditory nerve, we are satisfied that so-called concussion deafness is in many cases merely a passing phase in the temporary abolition of sensory impulses in a brain already anæmic as the result of physical fatigue and mental strain, the actual loss of hearing being induced by a sudden climax as it were—e.g., the bursting of a shell—accompanied as it is by a general atmospheric commotion, and the not infrequent burial of the soldier in the earthwork of his trench. Nine or ten cases have been observed by us,

of the deaf and dumb state, all recovered from within six weeks. The period of subconscious inertia following such injuries varies within wide limits, as also does the response of the special organ or organs involved. It is our belief that the abrogation of function is due not to an organic lesion, but to a temporary suspension of neuron impulses from the higher cortical cells of the central nervous system to the periphery. Our view is that the hiatus or synapse interfering with the flow of nervous stimuli is a central and not a peripheral one, for the reason that in so many of the cases of sudden blindness and sudden deafness no trace of any peripheral organic lesion was demonstrable, and moreover, the rapid recovery of so many of the patients we have observed is a strong argument that none was ever present.

What has struck us in many of the cases of so-called concussion deafness has been the presence of previous ear disease. This, we believe, has tended to throw the effects of the concussion more upon the sentient than upon the conducting segments of the organs of hearing. Illuminating evidence of the central origin of these functional disturbances is also obtained from an examination of soldiers who have suddenly lost the power of speech. Laryngeal examination demonstrates that these cases are not cases of hysterical aphonia, but that they are cases of neurotic or functional aphonia due to the sudden arrest of those volitional impulses which are necessary to produce speech. There is no paresis of the adductors as in true hysterical aphonia; there is a total inability to put the vocal cords in motion. Something has happened to prevent volitional impulses, a synapse somewhere, and probably in the cortical cells of the centres for speech, precluding that transmission of nerve energy which is requisite to start the machinery of speech. Such cases do well with rest, strychnine, and a judicious mixture of auto-suggestion and encouragement.

These thoughts and observations are but as pebbles on a rock-bound coast, a coast presenting difficulties to him who would explore inland; but to the man who succeeds in passing what at first sight appear impenetrable barriers, there is the reward of having added something to the sum total of human knowledge, of having done something for the benefit of mankind in general, and in particular for the welfare of those courageous men who at present so bravely and tenaciously defend our sea-girt home.

"On Fame's eternal camping ground
Their silent tents are spread,
And Glory guards with solemn arms
The bivouac of the dead."

Exhibit—German Cartridge with Bullet reversed.

By WILLIAM HILL, M.D.

THIS specimen was one of a large stock of rifle ammunition, similarly treated, found on a German sniper who was captured on a haystack near Ypres.

GROUP I.—FUNCTIONAL CASES.

Two Cases of Functional Aphonia (one including Functional Deafness) following the Bursting of a Shell in close Proximity to the Patient.

By HERBERT TILLEY, F.R.C.S.

THE first case was that of a man near whom a shell had burst on November 5, and from this time he had been aphonic till December 11. The second case had been buried by a "Jack Johnson" for four hours, and when he was dug out his voice had gone and he was also deaf. He remained thus for three months, and was sent into the hospital. In neither patient could any other disease be found, and inquiry showed that the voice was strong before the accidents. The first got well in response to a moderate intralaryngeal faradic shock. But in the case of the second, even a strong faradic shock would not restore phonation. As the patient refused to open his mouth, the laryngeal electrode was passed through the left nasal cavity into the larynx and the current passed until the resulting spasm induced marked cyanosis. On removal of the electrode the patient's voice returned and he spoke for the first time for three months. The hearing also was restored. These faculties have not since been lost, but his general condition may be described as that of profound neurasthenia.

Case of Gunshot Wound of the Neck with Laryngeal Symptoms for Diagnosis and Opinions as to Treatment.

By Coubro Potter, M.D.

G. B., aged 24, King's Royal Rifle Corps, sent from Millbank to the Metropolitan Ear, Nose, and Throat Hospital, suffering from complete aphonia. On September 14, 1914, he was hit over the head with a rifle, causing concussion of the brain. He was sent home to hospital, recovered, and returned to the Front, where, during a night attack on February 17, 1915, he was shot in the neck. The bullet entered 1½ in. below the lobe of left ear and came out about 2 in. from symphysis menti on right side. The patient intimates that from this date he could not speak. Previous history good.

On examination I found that the patient had left-sided paralysis of the cervical branches of his facial nerve, producing drooping of the angle of the mouth. He also had left unilateral paralysis of the hypoglossal nerve, which caused deviation and twisting of the tongue. Laryngoscopic examination was very difficult at first, owing to overhanging of the epiglottis and the nervousness of the patient. The true vocal cords did not approximate on phonation, nor did they move during breathing, but the arytænoids approximated during phonation. Subsequent examination gave a very much better view of the vocal cords, and at times the cords during phonation gave the impression of a certain range of movement. Nothing else of importance was noted except a slight amount of anæsthesia of the left side of the soft palate.

Functional Aphonia.

By J. F. O'Malley, F.R.C.S.

Six cases of this condition have come under my care, all having occurred in the trenches. In four, the voice returned on asking the patient to phonate with the laryngeal mirror in situ for the purpose of examination, and a mild application of the faradic current was effective in the other two.

Functional Aphonia.

By H. L. WHALE, F.R.C.S.

No. 11335.—Sudden aphonia in trenches without any injury. Perfect movement of cords, but in adduction he ceases to expire. Sudden recovery from neurosis in twenty-four hours.

DISCUSSION ON FUNCTIONAL CASES.

Sir FELIX SEMON desired to make brief reference to the case shown by Dr. Coubro Potter, which appeared to him to be extremely interesting and obscure. He was far from saying anything dogmatic about it, and only gave his impressions. He believed it to be a functional case, and for the following reasons: First, the entrance wound was very close to the left pneumogastric nerve, and the wound of exit was on the opposite side very much more in front. There was, however, no isolated paralysis of the left vocal cords. After a good deal of coaxing, the patient would haltingly say a few words in a whispering voice, a fact which was very suspicious, because if a man were merely aphonic from an organic lesion he would answer in a whispering voice questions put to him, but he would unhesitatingly answer. He did not by any means wish to say that the patient was malingering, but even now—though some months had elapsed since the injury—he gave a sort of stupefied response, as if unable to fully grasp what was being said to him. With this delayed response there was uncertainty in his movements. When he (Sir Felix Semon) examined the pharyngeal reflexes and asked the patient to lift his finger on the side touched, there was always a definite delay in the response. But the decisive factor in his belief was that there was *equally defective* movement in *both* vocal cords. If there had been actual injury to the left pneumogastric nerve, he would have expected to see complete paralysis of the left cord, or defective movement of it, in marked contrast to the condition on the other side. When there was an attempt to phonate, members would agree that the cords almost touched in the midline. Those facts, together with the general psychical attitude of the man, induced him to regard the case as functional.

Sir STCLAIR THOMSON said a striking feature of modern warfare was the number of functional cases which were freely reported and met with all over the country. In the old Laryngological Society, after the Boer War, someone showed a case of functional aphonia and it was looked upon almost as a curiosity in a man; and that case was freely quoted, and he even mentioned

it in his book. At the King's Sanatorium, at Midhurst, part of which was being given up to soldiers, the war would bring a large harvest of cases of tuberculosis among soldiers. Among the cases there now were three hearty-looking Grenadier Guardsmen, who were brought for his inspection as cases of tuberculosis of the larynx; each had functional aphonia, and the interesting point was that they had phthisis in the lungs. It was well to be warned, and not dismiss these cases of aphonia as merely functional; they must be examined for tubercular disease. One of these cases had functional dysphagia. They had been wounded in the retreat, and one had nourished himself with slops for four months, and talked in a very low whisper all the time. The functional aphonia was cured by suggestion.

Sir WILLIAM MILLIGAN said that on examining the case under discussion he had made up his mind it was functional; it was similar to a number of cases which he had been seeing for months. He agreed with Sir Felix Semon as to the man's slowness of response, which was an important, though not necessarily a diagnostic, factor. He could remember three cases in which there was a previous history of laryngitis, and where there were the remains of catarrhal laryngitis at the same time as this condition of so-called functional aphonia existed. He also supported Sir StClair Thomson's warning about not overlooking any tubercular element. He had seen only one example of this class of case with undoubted tubercle of both vocal cords, and a suspicion of chest tuberculosis. The functional element might well be engrafted on an organic basis.

Mr. TILLEY said that with regard to Dr. Potter's case, he was convinced that it was a case of functional loss of voice, because the patient was able to adduct the cord when saying "E."

Mr. W. D. HARMER said he had seen several cases of this kind at the First London General Hospital. Two impressed him considerably; one was that of a Guardsman who was sent to France, and arrived near to the firing line, and then seemed to have been thoroughly scared. He developed functional aphonia, and was sent home. The other man was wounded just beneath the jaw by a bullet which passed from one side of the neck to the other, and he came back wearing a tracheotomy tube, which was soon removed. He had no paralysis of any nerves, and the wound healed rapidly. Both cases were treated with the greatest care, and for long periods, but without improvement of the voice. He believed that as long as this war lasted such cases would be very difficult to cure, because the patients did not want to go back to the Front.

Dr. DUNDAS GRANT wished to draw attention to the very important remarks of Dr. Moure, of Bordeaux, who had had a large field for the study of the disorders arising from warfare. Dr. Moure said that "the men most often affected by bad weather or by the noise at the Front, had almost always old

auricular trouble, or present nasal or nasopharyngeal disability." This had also been very striking in the few cases which had come under Dr. Grant's own notice. He had under his care a young lieutenant with extreme neurotic disturbance and dreams amounting to most formidable nightmare. He had enlarged tonsils and a mass of adenoids. As soon as they were removed he improved with great rapidity. Another case was one of extreme deafness, but that was not in order to discuss here. When he was at Bordeaux he saw several cases of suddenly acquired deaf-mutism, which seemed to answer to no other treatment except re-education. Some of them reacted very quickly to the slightest stimulation, but others had too profound a disturbance of the synapses. A gentleman was most patiently carrying out this re-education in Bordeaux, and Dr. Grant thought such treatment might be necessary in this country.

Mr. STUART-LOW said that he had had a number of cases of loss of voice in soldiers, most of whom were about to be sent to the Front, but others had experienced this after being in the trenches. In nearly every instance there were anatomical disturbances in the nose and throat—deviated septum, enlarged tonsils, adenoids, &c.—and on these being operated upon the voice soon recovered. Mr. Stuart-Low had always made it a feature of his teaching at the Medical Graduates' College that electric treatment should not be undertaken in functional aphonia until gross obstructive conditions in the nose and throat had been removed by operation.

Mr. E. D. DAVIS said that two out of six cases he had seen were the subjects of functional aphonia grafted on to acute laryngitis, about which there could be no doubt. The aphonia persisted. In another type of case there was paralysis of the arytænoideus, and this seemed to be the most difficult to cure.

Dr. P. WATSON-WILLIAMS said it was very important to differentiate between true functional cases and those which were pseudo-functional and really due to organic mischief and weakness. A typical case, showing the influence of the nose condition on the larynx, was that of a soldier who for ten weeks had "functional" aphonia, and was brought to the speaker in consultation with a view to intralaryngeal faradisation. He explored the man's antrum, and found he had a latent antral infection with tenacious purulent secretion. This was washed out, and within two days the "functional" aphonia had disappeared. Many cases suspected to be purely functional, but which on investigation were found to be associated with incipient pulmonary tuberculosis, came into the same category.

The PRESIDENT expressed his agreement with Dr. Watson-Williams's remarks. Care should be exercised as to labelling as functional a condition which was a sequel of an organic lesion; there might be some arthritis or myositis remaining. These cases came up *apropos* the war, and it was

irresistible not to think of two elements: one was shock, fright and concussion due to the war, and the other was malingering. These elements must be borne in mind in almost every case. He observed that in Dr. Coubro Potter's case there were definite organic lesions of the hypoglossal nerve, the facial, and the superior laryngeal; and he was rather inclined to favour the view that the man's aphonia was also traumatic and not purely functional. A further report would be desirable at the next meeting.

Dr. COUBRO POTTER, in reply, expressed his gratitude to members for giving their opinion on his case. Though he had examined the man a good number of times, he was still unable to make up his mind as to whether it was functional or organic. At first he thought there was injury to one of the nerves, possibly the superior laryngeal; but that day he certainly moved his cords better and showed his larynx better too. But that might be because the many examinations had taught him to make his best effort.

GROUP II.—INJURIES OF NECK INVOLVING LARYNX, TRACHEA, &c.

Case of Laryngeal Stenosis following a Bayonet Wound treated by Intubation.

By E. D. DAVIS, F.R.C.S.

PRIVATE J., who was wounded in the shoulder, received a second bayonet thrust in the larynx on October 13, 1914, and was taken prisoner. The lower third of the thyroid cartilage was apparently destroyed, and the Germans inserted a large tracheotomy tube on December 4.

When seen at Millbank on March 3, 1915, he was wearing a large curved cannula inserted through the larynx with an upward extension of $\frac{3}{4}$ in. (the cannula is shown). He could neither speak nor breathe through the mouth. He had to write everything down to make himself understood.

At an operation on March 5 a high tracheotomy was performed and the thyroid cartilage was exposed. The cannula was removed and its extension was found to be buried in scar tissue. The cavity of the larynx was discovered by passing an intubation tube from above. The alæ of the thyroid cartilage were separated by $\frac{1}{2}$ in. of scar tissue at least $\frac{1}{4}$ in. thick. A large gap about 1 in. wide existed between the

larynx and the upper end of the trachea, in which the posterior wall of the trachea and larynx alone remained. A rubber drainage-tube was inserted from the tracheotomy tube below to the orifice of the larynx above, and the wound closed as far as possible, with the exception of the gap produced by the old cannula. The rubber tube was replaced later on by the intubation tube fixed by the midwifery forceps clip through the lower tracheotomy wound. The intubation tube and clip were described by Mr. Barwell in the *Lancet* on January 2, 1915 (a pattern is shown).

When the drainage-tube was in position no difficulty was experienced in swallowing, but after the insertion of the intubation tube the fluids made the patient cough.

The patient, who speaks well in a loud whisper, and the various tubes are shown.

Two Cases of Paralysis of the Left Vocal Cord.

By J. F. O'Malley, F.R.C.S.

Case I.—Private D., aged 23, returned from France in December, 1914, with slight shrapnel wounds of neck and head. On February 26, 1915, he came under my care with a history of having recently rejoined his unit, and being compelled during a route march to fall out, owing to dyspnœa, after walking a mile. He was voiceless for a fortnight following the injury, but speech returned and appeared to be quite normal since then. The healed scar of the skin wound in the neck was situated slightly to the left of the middle line, and opposite the upper border of the cricoid cartilage. The left vocal cord and arytænoid were immobile in the cadaveric position, indicating left recurrent laryngeal paralysis. The X-ray plate shows a piece of shrapnel which traversed the larynx and presumably injured the left recurrent laryngeal nerve. As dyspnœa is not a usual symptom of one-sided paralysis—although I have seen some cases in which it became marked on exertion—he was submitted to a very careful screen examination of the thorax by the X-rays, for a possible injury of the vagus or phrenic nerves, or other organic lesion. When at rest, the heart-beats and vessels appeared normal, but the right half of the diaphragm moved sluggishly. He was then asked to run up and down the hospital corridor until a feeling of dyspnœa was forthcoming, and he was then re-examined by the X-ray screen. The heart still beat regularly but more rapidly, and the right half of the diaphragm

became active, both indicating stimulation, response, and full nerve control. The only unusual condition discovered was some enlargement of the bronchial glands. He has been invalided from the Army.

Case II.—Private S., aged 27. This case, although arising under conditions of warfare, cannot be strictly classed as a warfare injury, such as those due to shot or shell, but is one of some special interest, and is therefore included. He lost his voice in the trenches in France, and was invalided home. He had no previous attack, and had no cold at the time of onset. On examination his left vocal cord was found to be in the cadaveric position, and he could only speak in a whisper. There was no injury to account for the condition. An X-ray examination disclosed a small aneurysm of the aortic arch. The history of onset in this case is interesting, in view of the fact that several cases of purely functional aphonia occur during the exciting stress of trench warfare. When contrasted with the previous case some noticeable points of difference arise in the symptoms. In both cases there was a lesion of the left recurrent nerve, followed by paralysis, the lesion being immediately followed by aphonia, which persists in the case of aneurysm but not in the case of injury, whilst the latter has, in addition, dyspnœa on exertion.

Traumatic Fixation of both Vocal Cords.

By J. F. O'Malley, F.R.C.S.

Private V. W. A shrapnel bullet entered at the lower border of the right inferior maxilla and traversed the neck from right to left; it was finally removed from the left supra-clavicular triangle. Aphonia was present from the date of injury. There was no history of hæmorrhage, dyspnœa, or dysphagia. On examination, the cords were found to be slightly more adducted than in the cadaveric position, and this was observed on several occasions and was maintained throughout forced attempts at deep inspiration and phonation. No wound or adhesion was discovered in the larynx. This patient is now on furlough.

Notes on the Case of a Soldier injured by a Horse's Kick.

By W. Stuart-Low, F.R.C.S.

A driver in the Royal Field Artillery was kicked by a horse on the right side of the neck. At the time of the injury, and for some days after, he complained of pain in the larynx. He subsequently became very hoarse, and sometimes had great difficulty in speaking at all; this continued for a month, when he was sent to the hospital. The entrance to the larynx was found to be very congested and somewhat swollen, but felt quite soft when palpated with the finger through the mouth. The vocal cords were seen to be moved normally, but were very congested. Dry-cupping was applied over the crico-arytænoid and thyro-hyoid spaces on both sides of the larynx simultaneously. The vocal cords and entrance to the larynx could be seen in the laryngeal mirror to become quite pale, and the swelling lessened under the action of the suction. The voice at once improved, and, after a few applications, in the course of a few days returned to normal.

Shell Wound of Neck.

By William Hill, M.D.

Private X., who suffered from marked dysphonia, was seen on January 21, at Torquay Red Cross Hospital. A fragment of shell had passed transversely across the neck two months previously; wound at entrance and exit healed; thickening of alæ of thyroid cartilage and of soft tissues superficial to larynx. When first wounded he had coughed up blood. Mirror examination showed a tumour-like mass, mostly above and to the right of the anterior commissure, in the region of attachment of left ventricular band to the right ala of the thyroid cartilage; the latter had probably been fractured by the fragment in its passage transversely through the neck. A similar injury was reported at the February meeting of the Section by Mr. Buckland Jones.

Tracheal Obstruction due to long latent Cervical Abscess following Wound by Shrapnel Fragment.

By William Hill, M.D.

Captain S. was wounded in October by a fragment of shell passing transversely through the neck. On arrival at Torquay about December 20 the wound of entry and exit had healed, but the patient had an up-and-down temperature and a recrudescence of pneumonia. There was evident narrowing of the trachea with expiratory stridor. When examined by Mr. Worthington on December 24 there was no laryngeal paralysis; though the tracheal narrowing caused stridor there was fair entry of air, and in view of the patient's weak and critical condition from pneumonia it was considered that tracheotomy would merely ensure a fatal result. The possibility of deep abscess either in the neck or in the mediastinum was considered, but there was no positive physical evidence of a cervical abscess. On January 20, I was asked to visit the patient, but before my arrival at Torquay a tracheostomy had to be hurriedly performed during the night by Dr. Payne for the relief of serious increase of dyspnœa. On incising the trachea a large intramural abscess (in the lateral wall of the trachea) under tension was struck; a Fuller's bivalve cannula was inserted. When I examined the patient two days later he was breathing easily through the cannula, but complained that it caused pain. I found that a Durham cannula was quite comfortably borne on account of its length being better adapted for a case in which the tissues of the neck superficial to the trachea were considerably tumefied. It was found that although the abscess had been evacuated, stridor returned on removing the cannula. The stridor, however, was now *inspiratory*, and on mirror examination there was seen well-marked abductor paralysis on the same side as the abscess; this latter was not observed when an examination was made by a laryngologist one month before. The patient's condition was not such as to warrant my making an endoscopic examination. [By coincidence this patient, who had not been seen subsequently to January by Dr. Hill, was shown at the meeting by Sir StClair Thomson; *see* next record, p. 125.]

Case of an Officer shot through the Neck.

By Sir StClair Thomson, M.D.

The injury resulted in complete stenosis, requiring tracheotomy. Restoration of the lumen of the larynx was effected. The patient is still under treatment with intubation tubes.

DISCUSSION.

The President, referring to this case, said he had previously seen the patient, and he thought the abscess was due to traumatic perichondritis and a necrosed tracheal cartilage, which was relieved directly tracheostomy was done. The abscess had been lying latent. The inflammatory thickening was now higher up than his notes led him to believe when he examined the case in January, and at that time there was no laryngeal paralysis, though there was now distinct loss of movement in one cord. In his other case (p. 123) the ala of the thyroid was fractured. A similar one was shown at a previous meeting by Mr. Buckland Jones, which like this one presented the appearance of a growth.

Sir StClair Thomson said he was not aware that the President had been connected with the case he (the speaker) had shown. These cases of stenosis of the larynx were very tiresome to treat. When this officer came, he had what seemed to be a web immediately below his vocal cords. One cord was more or less fixed, the other moved; but there was complete stenosis, and the patient breathed entirely through the tracheotomy tube. The history showed how the condition had spread up from the trachea into the larynx and subglottic tissue. It showed the wisdom of doing tracheotomy low down. High tracheotomy should be ruled out, except in cases of urgency. In this case there was a difficulty in finding a thoroughfare, but under direct laryngoscopy a passage was obtained. There was fair dilatation except at the last part. This dilatation was being attempted by long intubation tubes, suggested by Rogers, and used in this country by Barwell and E. D. Davis. As long as there was a tracheotomy tube in, the man bore his intubation tube quite well, but to dilate the last part the tracheotomy tube had to come out. He would be glad of suggestions as to whether the process could be persevered with, and as to methods of securing it by clamps, as described in the *Lancet* by Mr. Barwell early this year.

The President said that in this case he did not do the tracheostomy; it was done before he could get to Torquay. He could not say positively it was an intramural tracheal abscess. He gathered that Dr. Payne did not consider until he cut down that there was an abscess present in the trachea or

elsewhere in the neck. He (Dr. Hill) imagined that the cartilage had necrosed, therefore it was likely that the cricoid was also involved in the abscess and in the perichondritis. The difficulties of making a diagnosis in such a case were very great without doing a tracheoscopic examination, and the patient's condition at the time contra-indicated that procedure.

Mr. HOPE said that apparently the reason the patient had the long tube out was that he began to get frightened; he wanted to cough, and thought that if he did he would cough the tube out; it was not that he had not enough air. He considered there was a difficulty in getting the clamps on. In the President's case it was a rubber tube which kept the fibrous stricture open very well, and such might be of use to this patient.

Mr. HARMER said that he was now treating two similar cases with Schimmelbusch tubes, and both were doing well. The only difficulty was to get the upper segment of the right length, this requiring careful measurement in each case. The two portions were introduced separately, and afterwards fixed together by a screw. For cleaning it was only necessary to unscrew the cap and clean the outer part of the tube, as the inner portions were often free from mucus. The tubes were made of silver, and in some instances could be worn for a month without being changed.

Dr. DUNDAS GRANT said that Thost, of Hamburg, had devised some ingenious plugs which were adapted for introduction above the upper part of the tracheotomy tube; they were solid. He saw Dr. Moure using a very simple arrangement, namely, a thick piece of drainage-tube with a hole cut in the side, through which the tracheotomy tube was passed. The continuation upwards had the pliability of the india-rubber. Dr. Moure had five cases of laryngostomy since the war began. At the first stage of the operation he plugged with a "basket" of gauze, the whole smeared with oxide of zinc in vaseline 1 in 15. It was very easy to remove in two or three days, and later it was replaced by the tube above described. The softness of the rubber enabled the upper part to be coaxed up into the larynx after the lower part was introduced with the tracheotomy cannula.

Dr. P. WATSON-WILLIAMS understood from Sir StClair Thomson's remarks that in this case a high tracheotomy was performed, and asked why he recommended a low tracheotomy in such cases. A tracheotomy at the seat of the abscess seemed to favour drainage of the suppurating tissues, and rather helped manipulations in the constricted part of the larynx. His own experience was that vulcanite or metal instruments did not excite so much local reaction as india-rubber tubes, and he had not been favourably impressed with the use of rubber in inflammatory stenosis.

Mr. E. D. DAVIS said he had struggled with such a case at Millbank for two months. The man was wearing an intubation tube with Delavan's clip through a high tracheotomy. At first swallowing was difficult, because fluids

entered the intubation tube. Since the tube was altered, the man swallowed much better. The clip was applied in the same way as midwifery forceps, first one blade, and then the other, through a large aural speculum. The man had had a huge German tracheotomy tube (shown) inserted into the larynx, and an extension tube projected upwards into the thyroid cartilage, but it went into scar tissue, not into the lumen of the larynx. The man could not then speak, but now that he was wearing the intubation tube he spoke in a loud whisper, and was doing very well.

Mr. HOPE asked whether members would give their experiences with the large vulcanite intubation tubes. Several of enormous sizes were sent down, but on trying to use them the patients got perichondritis and fixation of the cord, with pain in the larynx.

The PRESIDENT said he had one such tube as Mr. Hope mentioned, but he lacked the courage to use it.

Sir STCLAIR THOMSON, replying to Dr. Watson-Williams, said this tracheotomy was excellently performed, and it was a low tracheotomy. This case showed the importance of doing a tracheotomy lower than was usual in all inflammatory conditions. When Dr. Hill saw it the larynx was clear, but when he (the speaker) saw it the cicatricial tissue was spreading up to below the cords. There was a great tendency for inflammatory tissue to contract in the subglottic region.

The PRESIDENT said the remarkable point about the case was that the abscess was latent for two or three months; and, when seen by Mr. Worthington, of Exeter, a month before the speaker saw him, the former said he concluded there was no definite evidence of abscess. The reason was that the tissues were so brawny after these wounds, even when they had healed up, and the patient being ill with pneumonia and looking as if he would not stand an operation, it was a difficult matter to advise exploring on the chance of finding a latent abscess. There was, however, such a latent abscess in this case, and the patient nearly died as the result of its not being diagnosed and dealt with more promptly.

GROUP III.—INJURIES TO VERTEX, FACE, JAWS, NASAL CAVITIES, &c.

Bullet Wound in the Head.

By J. F. O'Malley, F.R.C.S.

E. H., a Belgian soldier, aged 30. A bullet entered the vertex to the left of the middle line, about 1 in. in front of the surface marking for the fissure of Rolando, apparently traversing the left half of the brain and found exit from the skull by passing through the body of the sphenoid. It entered the nose by the roof of the left posterior nares, passed through the middle turbinate and inferior meatal floor, slightly injuring the inferior turbinate. It finally passed through the anterior part of the hard palate and floor of the mouth, the wound of exit being situated in the neck to the left of the symphysis menti and behind the horizontal ramus of the lower jaw. He came under my care several weeks after the injury and looked extremely blanched. There was a history of several recurrences of bleeding, which were at times immediately arrested by inserting gauze, soaked in adrenalin, for about 2 in. inside the left nasal fossa. At other times this packing was not effective and slight oozing continued for hours. On examination, an adhesion of the left inferior turbinate to the meatal floor was seen, with a granulation area on the latter, which bled easily but not profusely when touched with a probe. On posterior rhinoscopy a ragged wound could be seen in the roof of the posterior nares, which I now believe was the source of much of the hæmorrhage. A post-nasal plug of gauze, squeezed out of adrenalin, was inserted against the posterior wound and held in position by a stout silk thread passing through the nose, the latter being fixed externally to a piece of rubber tubing. This plug was left in situ for twenty-four hours. The bleeding ceased immediately and has not since recurred. His left antrum was dark on transillumination, and an X-ray examination showed opacity of this cavity together with that of the lower ethmoidal region (plate shown). The antrum was punctured and washed out, but the fluid returned quite clear. Organised blood-clot was probably the cause of the opacity. Lieutenant Kiep, who examined his eyes, reported primary optic atrophy on the left side, with complete loss of vision. The bullet

had apparently injured the optic nerve in its passage through the sphenoid. After the arrest of hæmorrhage, he improved rapidly in colour and weight and has now rejoined his regiment in the fighting line in Flanders.

Two Cases of Intranasal Adhesions.

By J. F. O'Malley, F.R.C.S.

Case I.—Lieutenant M. Some pieces of shrapnel entered below the patient's left eye, traversed the upper part of the left antrum, left and right nasal passages and septum, and lodged in the right pterygoid region, where they still remain. The left inferior turbinate had formed extensive adhesions to the septum, whilst a similar but less severe condition existed on the right side. The septum was markedly deviated to the left side, which, in my opinion, was a large factor in predisposing to the formation of the more extensive adhesion. I have noticed this influence in other cases also. On blowing the nose, he complained of air rushing into his left eye. The conjunctiva was injected. An X-ray examination showed opacity of the left antrum and destruction of the tip of the right coronoid process. The adhesions in the nasal fossæ were divided, but rapidly re-formed on the left side, although rubber splints were used for six days. They were finally cured by means of a submucous resection. The antrum was punctured and found free from pus, organised blood-clot being the probable cause of the opacity to X-rays and trans-illumination. The nose has healed perfectly, the eye symptoms have disappeared, and no further trouble is experienced in the lachrymal duct.

Case II.—Lieutenant P. A bullet had entered the anterior half of the left eye and side of the nose, leaving a large stellate wound of exit below the inner canthus of the right eye. The latter was untouched, but the left eye was destroyed. As he reached hospital on the third day after injury, the intranasal œdema was still very marked and was allowed to subside, to ascertain the degree of permanent obstruction, before any attempt was made to look for or treat adhesions. After a fortnight the swelling subsided and the left passage was quite free, but an adhesion had formed in the right, above the inferior turbinate. This was removed by a punch forceps, which appears to me to be better than scissors in these cases, and there was no further trouble.

If no bones are displaced or no marked external deformity exists, calling for some immediate operative interference, I believe it to be sound practice to treat injuries of the nose on an expectant plan, although marked nasal obstruction may exist in the early stage. This obstruction is chiefly traumatic œdema, and will subside if no manipulations are permitted. I have had several cases of severe nasal injuries due to falls off horses and kicks from them in young soldiers learning to ride, and have come to the conclusion that the less interference in such injuries the better.

Bullet in Pterygoid Region of Skull.

By Walter Howarth, F.R.C.S.

This case was not seen until eight weeks after the injury. At that time there was a purulent discharge from both nostrils with almost complete nasal obstruction and considerable swelling in the right parotid region. The chief disability was that the mouth could not be opened wider than 1 in., and that there was much pain in the region of the right temporo-maxillary joint. The point of entry of the bullet was in the cheek just above the left canine fossa. The left antrum was found to be disorganised and suppurating, and there were several adhesions between the outer wall of the nose and the septum. There was a ragged hole in the septum and in the nasal wall of the right antrum, which was also suppurating. At the first operation both antra were drained and several fragments of lead removed from the septum and right antrum; a large portion of the anterior wall of the right antrum was removed, but the bullet itself could not be reached by this route. The skiagram showed it as an horseshoe-shaped mass internal to the right temporo-maxillary joint and close up to the base of the skull. When the antral suppuration had subsided, the bullet was extracted by the external route. For this purpose a curved incision was made over the temporo-maxillary joint towards the external angular process, a portion of the zygoma was removed and the temporal muscle retracted forwards. The exploration was then continued through the sigmoid notch until the bullet was reached and removed. The conical point of the bullet was intact, but at the posterior end the casing was split and the lead mushroomed out and bent round. Recovery was uninterrupted, and in a few weeks the power to open the jaws widely was regained.

Case of an Officer shot through the Cartilaginous Part of the Nose.

By Sir StClair Thomson, M.D.

This injury led to almost complete occlusion; the airway was restored by submucous resection and division of adhesions.

Cases of Injury principally to Lower Face and Mandible.

By H. L. Whale, F.R.C.S.

Special attention is drawn to a few remarkable cases.

No. 6801.—A man was shot through the neck in front of the sterno-mastoid, the bullet emerging on the opposite side of the face. It missed the internal carotid, although it tore the tonsil away.

No. 6317.—In another case, of which the skiagram was shown, the pre-maxilla was blown out, and now stuck out like a rabbit's jaw. It went back well, and stayed back, as shown by the second skiagram, taken after operation.

He had five cases of wounds from the nose down to the thyroid region, of varying severity.

One was open from the columella nasi to the thyroid. The lower part of the face and upper part of the neck were turned outwards like an open door, and the wound was large enough to admit the hand. The patient died in twelve days.

Another had a vertical wound from the inner canthus, through the antrum, into the mouth, so that it was possible to look directly into the spheno-maxillary fossa. It healed up, as most of them did after plastic operations.

In one case two incisor teeth were buried between the alveolus and the soft tissues of the chin. In another case a piece of jaw 3 in. long was turned completely round, so that the contained teeth pointed downwards.

In another case, which recovered with partial speech and fair swallowing-power, the jaw was so comminuted that both rami, from angle to angle, had to be removed.

The following notes are a brief synopsis of the cases in order:—

No. 978.—Bullet wound transversely through extreme posterior part of both antra. Practically no symptoms. Septum, ethmoid and sphenoid were all untouched owing to the slightly varying inclination of the vomer; this is only possible in certain skulls, not in all.

No. (?).—Vertical shrapnel wound from mouth to thyroid cartilage. Thirty-hour journey to base in train, during which time he fed himself through self-passed rubber tube. Recovery.

No. 7451.—Vertical shrapnel wound from mouth to hyoid bone. Extensive comminution of lower jaw. Plastic operation; drainage. Good result.

No. 298.—Both eyes destroyed by transverse shrapnel wound. Operation: Both ethmoids cleared out through double eyebrow incisions. Sent home well, but blind.

No. 6801.—Bullet wound: Entrance posterior border of left sterno-mastoid, 4 in. below lobule of ear; exit just above the angle of right lower jaw. Left tonsil partly torn away. Internal carotid artery untouched. Practically no symptoms.

No. 2473.—Shrapnel wound of left antrum; tetanus. Treatment: Subcutaneous injection of serum, large subcutaneous doses of 2 per cent. carbolic acid, and wound dressed with brine. Latent period was fourteen days. Recovery.

No. 7828.—Screw-top of time-fuse impacted in floor of nose. Removed by reflecting muco-periosteum from hard palate. Good healing.

No. 7746.—Shrapnel. Enormous gaping wound from columella nasi to thyroid cartilage; upper and lower jaws comminuted; recurrent hæmorrhage from left coronary artery. Fed through tube. Lived until the thirteenth day.

No. 6317.—Dislocation of premaxilla by bullet. Replaced (*vide* skiagram).

No. 5120.—Shrapnel wound of right antrum, making it possible to look directly into spheno-maxillary fossa. Plastic operation. Home with very ugly scar.

DISCUSSION ON INJURIES TO HEAD, FACE, NOSE, AND JAWS.

Mr. ANDREW WYLIE remarked that neither Sir W. Milligan nor Major Westmacott had mentioned that the injuries incurred during the war were nearly always on the left side of the face, neck, and body. It had also been remarked that it was nearly always the left eye and the left limb (arm or leg) which were injured. This was explained by the position soldiers took to shoot or to use their bayonets, and nearly all the cases on the programme that day had had bullet wounds in the left side of the neck or face. Soldiers who had been shot in the right side had, as a rule, been retreating, and had naturally turned towards the right side.

Mr. O'MALLEY said he had only sent notes of his typical cases; he was sorry he could not send skiagrams. The point mentioned by Mr. Wylie had struck him also; all his own cases had been wounded on the left side of the face. With regard to bullets turning round, he had a very interesting case a few days ago, in which a bullet entered through the left ala and floor of the left side of the nose. He found a large wound between the hard and soft palate. X-ray examination showed the bullet lying transversely in front of the odontoid process. The interesting problem arose in this case as to whether the general surgeon should approach it through the neck, or should the nasal surgeon through the post-nasal route. In the end they did it in combination. An opening was made behind the sterno-mastoid, but it was the base of the bullet which was thus reached, and every time an attempt was made to free it, it seemed to move farther away. He eventually delivered it by using his finger. The bullet was turned round as if it had entered sideways.

Dr. VINRACE desired to refer to a case which, though it occurred in civil practice, bore on the present discussion. A young lady was shot with a revolver pistol on the right side of the face, the bullet entering in front of the right external auditory meatus, and emerging on the left side of the nose without causing any material damage. Healing took place in a very few days. A romantic aspect of the case was that after leaving the lady the bullet impinged the right shoulder of her lover, penetrated the cloth, passed down the lining of the coat, and was next morning brought by the gentleman in great triumph to the hospital.

Dr. DUNDAS GRANT said that at the hospital of "Val de Grâce," Paris, there were wards full of cases under this heading of the subject; they presented terrible disfigurements of nose, face, and jaw. With regard to the nose, he saw some of the stages of the operation practised by M. Morestin. The typical operation in some of the cases in which the nose was driven in and the nostrils turned up and looked forward was to cut across just below the nasal bones, and turn the upturned portion of nose downwards, leaving a large gap, which was stuffed with gauze. After a day or two india-rubber

drains were put into the nostril, with weights attached to keep that portion of nose hanging down. M. Morestin then implanted a portion of cartilage from the eighth rib into the forehead, and in due course fashioned the upper part of the nose from the forehead, and brought it down on to the piece which he had originally detached. It was a combination of two previously described operations, and the result was a great improvement on the disfigurement which men would otherwise show. The passages of the nose were made quite free.

The PRESIDENT, in calling upon the authors of the opening paper to reply, said he thought the holding of this discussion had been justified. It was generally felt by the Council that if there was to be a set discussion this year it could be on only one subject, namely, warfare injuries; and the only misgiving had been on the question as to whether a sufficient amount of material would be forthcoming. The experiences of members of the Section as set forth in the agenda, together with the subsequent discussions, would be of permanent instructional value for future guidance and the holding of the symposium had been thoroughly justified.

Major WESTMACOTT, in reply, said the point made in the paper, that a bullet, if it were not doing harm, should be left in situ, had not been controverted. The patients under discussion were fortunate in not being septic when seen, as a rule. If any were septic, some peroxide of hydrogen and iodine had put them into a good condition to be dealt with by operative treatment. One case, which might have been mentioned in the paper, was that in which the whole of the maxilla was removed by a piece of shell, the lower jaw of the opposite side was also split at the symphysis and turned outwards and the whole of the right cheek laid open. It had healed remarkably by trimming up and keeping clean. With regard to the functional question, their cases were purely functional ones, they were central rather than peripheral, and there was no ascertainable injury. In cases where there was injury to the cartilage of the larynx there was prolonged inflammation, during which time small pieces had necrosed away, but there was eventual healing.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE EIGHTH

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1914-15

MEDICAL SECTION

LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1915

Medical Section.

OFFICERS FOR THE SESSION 1914-15.

President—

SAMUEL WEST, M.D.

Vice-Presidents—

ARCHIBALD E. GARROD, M.D., F.R.S.
PERCY KIDD, M.D.

Hon. Secretaries—

JOHN FAWCETT, M.D.
CHARLES R. BOX, M.D.

Other Members of Council—

W. CECIL BOSANQUET, M.D.
J. CHARLTON BRISCOE, M.D.
Sir JOHN F. H. BROADBENT, Bt., M.D.
A. J. HALL, M.D.
HENRY HEAD, M.D., F.R.S.
ROBERT HUTCHISON, M.D.
CYRIL OGLE, M.D.
H. BATTY SHAW, M.D.
F. PARKES WEBER, M.D.
F. J. WETHERED, M.D.

Representative on Library Committee—

G. NEWTON PITT, M.D.

Representative on Editorial Committee—

JOHN FAWCETT, M.D.

MEDICAL SECTION.

CONTENTS.

February 23, 1915.

PAGE

Multiple Pulsating Tumours secondary to Hypernephroma. By Frederick Taylor, M.D. 1

By F. Parkes Weber, M.D.

(1) Bilateral Hypernephroma, with Secondary Thrombosis of the Inferior Vena Cava and Terminal Uræmia 6

(2) Latent Addison's Disease revealed by Death from Pneumonia and Empyema 11

April 27, 1915.

By Arthur J. Hall, M.D.

(1) A Case resembling Acute Lymphatic Leukæmia, ending in Complete Recovery 15

(2) Notes on some Cases of Myelogenic Leukæmia 19

An Instance of Long Quiescence of Leukæmia. By F. Parkes Weber, M.D. ... 28

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
GREAT TITCHFIELD STREET, OXFORD STREET, W.

Medical Section.

February 23, 1915.

Dr. Samuel West, President of the Section, in the Chair.

A Case of Multiple Pulsating Tumours Secondary to Hypernephroma.

By Frederick Taylor, M.D.

The following case has many points of interest; these are—the nature of the primary tumour in the kidney, the occurrence of secondary growths, their pulsation, and the murmurs audible in them.

A sailor, aged 59, was admitted to the Seamen's Hospital, Greenwich, on November 25, 1907. He was found to have pulsating swellings of the right elbow, the right shoulder, and the right gluteal region, together with hæmaturia. The first occurrence of hæmaturia was in January, 1907, when it continued for three days. At first micturition was painful and clots of blood were passed, but with their passage the pain ceased. The urine continued to be bright red and the colour was evenly distributed. There was no pain shooting to the end of the penis and no pain in the loins or in the testes. The urine was then free from blood for two months. Hæmaturia again occurred; and every five or six weeks since then the patient had had similar attacks lasting two or three days. On November 22 the stream of urine was thought to be a little smaller, but no pain was felt. On November 24, the day before admission, the patient was unable to pass urine until 1 p.m., when a few drops of blood-stained urine and clots were passed, and in the afternoon a large quantity of very dark urine. The swelling on the right buttock was first noticed twelve months ago, that on the elbow six months ago, while that on the shoulder had not been recognised, though the part was painful. Three weeks before the first occurrence of hæmaturia he had a fall on the right shoulder, elbow, and hip, and he had had frequent pains in these parts since that time. The pain in the right shoulder increased two days before admission.

The most obvious of the swellings was that involving the right elbow-joint;

it was an elastic, rounded swelling, and though affecting more the lower end of the humerus than the upper ends of the bones of the forearm it had very much the appearance of a moderately swollen joint. The movements, however, were not much limited, and both flexion and the radio-ulnar movements when carefully performed were not painful. The swelling was, however, tender to pressure, especially on its outer side, was soft and elastic, and pulsated distinctly. On auscultation a rather loud systolic bruit was heard. The right arm was wasted, but the muscle was capable of normal action. The tumour of the shoulder formed a rounded swelling of the acromial end of the clavicle and of the acromion itself, about 2 in. in diameter, and projecting above the normal level from $\frac{1}{2}$ in. to $\frac{3}{4}$ in. It was soft, elastic, tender on pressure, and pulsating. A systolic murmur could be heard in it. The shoulder-joint was painful when moved.

The swelling of the right gluteal region was situated behind the trochanter, forming a flat elevation, projecting at its centre not more than 1 in. beyond the normal level of the skin. Its limits were difficult to define, but the pulsation associated with it could be traced upwards nearly to the iliac crest, backwards to the edge of the sacrum, and downwards to the level of the inner end of the trochanter. A systolic bruit was heard over this area. The right hip was painful on movement, and pressure behind the trochanter caused pain.

The course of the illness was uniformly downwards. The tumours gradually increased in size, and the patient became increasingly paler and weaker. The hæmaturia, however, was intermittent, and even for long periods absent. In January, 1908, he had pain in the left leg, but nothing could be detected to account for it. On March 1 the tumour of the shoulder extended, as estimated by the pulsation, from the spine of the scapula to just beneath the coracoid process, and measured 5 in. by $3\frac{1}{4}$ in. The tumour at the elbow was 5 in. in length and $10\frac{1}{2}$ in. in circumference. The vertical extent of the gluteal tumour was as before, and antero-posteriorly it extended from the anterior superior spine of the ilium to the tip of the coccyx. Blood was found at intervals in the urine, sufficient to colour it bright or deep red, and sometimes clots were passed. On these occasions strangury and pain at the end of the penis were present, but there was no pain in the loins. Beyond the presence of the blood corpuscles and the corresponding albumin the examination of the urine was negative. In view of the presence of multiple growths in connexion with the bones, the urine was examined more than once for Bence Jones's protein. It was stated to be present on the first occasion, but subsequently it was never found, and I think the first observation must have been erroneous. The patient was pallid and wasted, looked much older than his age, and had the appearance of one suffering much pain. The lungs appeared to be normal. The heart's impulse was $\frac{3}{4}$ in. outside the nipple line; there was some accentuation of the second sound, and a systolic murmur at the third left costal cartilage. Nothing was detected in the abdomen.

On June 16 the tumour of the elbow measured $11\frac{1}{2}$ in. in circumference, that of the shoulder had increased to $6\frac{1}{2}$ in. by $3\frac{1}{2}$ in., and that of the right

buttock to 10 in. by 9¼ in. Three weeks later a new pulsating area was discovered at the posterior extremity of the left iliac crest. On July 31 all the swellings were larger. The patient died on September 18, 1908.

The following account of the necropsy and the histological report are by Dr. Lucius Nicholls, then Pathologist to the Seamen's Hospital, and now Resident Surgeon to the Victoria Hospital, St. Lucia, Windward Islands, W.I. :—

Necropsy.—The body was that of an emaciated, aged man. Four distinct hard swellings were visible upon the body surface; the first was a large oval swelling situated at the outer end of the clavicle; another was on the lower end of the humerus and involved the elbow-joint. The bone here was obviously fractured. The third and largest tumour was in the right gluteal region on the inner part of the iliac bone. The fourth was in the right lumbar region and appeared to involve the fourth and fifth lumbar vertebræ. These tumours were dissected out. The one on the outer end of the clavicle had caused fracture of that bone: it was 3 in. or 4 in. long and 2 in. broad, and it involved the upper border of the scapula. The growth on the lower end of the humerus was about the same size; it had also caused fracture of the bone and involved the upper ends of the radius and ulna. The gluteal tumour had involved the wing of the ilium and the adjacent half of the sacrum. This tumour was about 4 in. square. The sections of these tumours had the appearance of a lobulated structure, of a dark, mottled, bloody colour. This was due probably to extreme vascularity and hæmorrhages into the substance. Careful examination of the other skeletal structures revealed no further growths, and no bone broke on heavy manipulation.

The left kidney measured 5½ in. vertically. The upper third was occupied by a spherical tumour 2½ in. in diameter; it was lobulated and had a definite capsule. On section it presented hæmorrhagic areas. Throughout the rest of the kidney, except 1 in. at the lower pole, were scattered growths of the same kind, from the size of a pea to that a filbert. The left suprarenal capsule contained a small separate tumour. The right kidney and suprarenal capsule were natural. The lungs were dry and silky, and contained numerous small nodules scattered through them, ranging from the size of a split pea to that of a small walnut. They could be easily shelled out. The cardiac valves were hard and sclerotic and the inner coat of the aorta was in an advanced stage of atheroma. The heart itself and the abdominal organs, other than the kidneys and suprarenal capsules, were normal and free from growths. The testes were normal.

Histological Report.—The tumour is a so-called hypernephroma. In the present case the tumours are very degenerate, and much hæmorrhage has taken place into their substance. They have, however, the usual reticular appearance of hypernephromas, but the cells show badly and the nuclei are darkly stained. (The tumours of the kidney, humerus, scapula, and ilium, and histological sections, were shown at the meeting.)

There seems to be no doubt from the histological examination as well as from the character of the recurring growths that the tumours in this case were identical with those which have been called hypernephroma; but I am afraid that the case contributes little to the discussion as to the origin of these tumours—that is, to the solution of the question whether they arise from suprarenal remains in the kidney or are only one form of carcinoma of the kidney.

Mr. Wilfred Trotter[1] says that a "true carcinoma of the kidney apart from hypernephroma is a very great rarity or possibly does not exist." Sir John Bland-Sutton says[2] "the opinion is gaining ground that the majority of tumours classed as hypernephroma are carcinomatous, arising in the cells of the renal tubules, and I share in this opinion. The classification of malignant renal tumours is a difficult matter." In the French work on diseases of the kidney by Jeanselme, Weil, Chauffard and Laederich, the authors describe epithelioma with dark cells and epithelioma with clear cells. They admit that when the clear cells have a tubular arrangement they bear a close resemblance to the cortical layers of the adrenals; and, indeed, that they are sometimes derived from adrenal remains; but that all clear-celled renal tumours have this origin they say is not true.

The case illustrates very well the metastasis to other organs, which is known to be a feature of hypernephroma. Though the lungs, liver, bowels, and lymph glands are sometimes the seat of secondary tumours, it is especially the bones and muscles which have been observed to suffer; and these were the chief sufferers in the present case. Apart from the nodules in the lungs, these secondary tumours were four in number, involving in all cases the bones—namely, the right clavicle, the right humerus, the right ilium, and the fourth and fifth lumbar vertebræ.

Mr. Trotter in his paper makes it a point of practical importance that a hypernephroma often produces only one secondary growth. In a group of cases described by Dr. R. Hutchison, sarcomatous growths of the adrenals in children were accompanied by secondary growths in the skull only, but apparently these were not hypernephromas proper.

The fact that these growths pulsated requires some explanation. As a physician, I have been haunted all my professional life by the

[1] *Lancet*, June 5, 1909, p. 1582.

[2] "Tumours," p. 105.

warning that a pulsating abdominal or thoracic tumour may be, in occasional cases, not an aneurysm but a pulsating sarcoma. But I have rarely, if ever, been compensated for my fears by the discovery of such a growth. In my experience pulsating sarcomas are exceedingly rare. No mention is made of the probability of pulsation in these secondary tumours by Mr. Trotter. In Dr. Hutchison's group of cases no sarcoma of the bone appears to have pulsated.

Cancers of the liver, sarcomas and lymphosarcomas of mediastinal glands, and other large masses of growths rarely pulsate. I saw, however, recently a renal growth which extended forwards in the upper part of the abdomen across the middle line, and this portion, lying as it did across or in front of the aorta, pulsated, and the pulsation appeared to be in the growth itself.

I presume pulsation must be dependent upon a high degree of vascularity, and the abundant hæmorrhage in these tumours found after death suggests that such a condition was here present. The liver under special circumstances pulsates—that is, when by regurgitation through the tricuspid valve the right ventricle is able to impress the force of its contraction upon all the ramifications of the hepatic system of veins. In relation to the large hepatic vein when its current is reversed it is a highly vascular organ; but in relation to the small hepatic artery it is by no means highly vascular; and in health it does not pulsate sufficiently for clinical recognition. It is interesting to note that the liver, with its 60 oz. of weight, has an artery only half the size of the artery of the spleen, with its weight of under 10 oz. And yet the spleen, with its relatively huge blood supply and wide blood channels, is not clinically, even when enlarged, seen or felt to pulsate.

A similar interest attaches to the murmur which was heard over the pulsating growths. Blood is in health coursing rapidly and abundantly through muscles, the liver, and other structures, and yet there is no sound audible with ordinary means of investigation. What is the special condition of a vascular tumour which leads to the production of vibrations of sufficient amplitude to be audible?

Bilateral Hypernephroma, with Secondary Thrombosis of the Inferior Vena Cava and Terminal Uræmia.

By F. Parkes Weber, M.D.

The patient, P. M., aged 49, a commercial traveller, was admitted to hospital on October 19, 1914. He was feeble and emaciated, with distended abdomen and œdema of the legs. He said that he had enjoyed good health till the end of January, 1914, when he was in a taxi-cab accident and received a knock on the right side of the abdomen. The right side of the abdomen was considerably bruised, and in February, 1914, his legs began to swell. No past history of syphilis was obtainable.

In the hospital: There was great enlargement of the superficial veins over the distended abdomen, the circulation of blood in these veins being in an upward direction (towards the thorax). In addition to ascites moderate enlargement of the liver could be made out, but what was clinically supposed to be enlargement of the spleen was doubtless in great part the enlarged left suprarenal gland and left kidney. The urine contained a trace of albumin, and microscopical examination showed the presence of some erythrocytes and granular tube casts (the specific gravity, noted in the early part of November was 1016). The pulse-rate varied between 80 and 100, and the respiration between 24 and 40. There was never any fever, except between October 27 and October 31, when the temperature reached about 100° F. in the evenings.

On November 13 the gradual onset of a condition of coma was first noted. This condition was associated with a subnormal temperature, and diminution in the quantity of urine, which became almost complete anuria on November 14. In spite of treatment directed towards the suppression of urine the uræmic coma persisted till the patient's death on the night of November 15. Up to November 11 the daily quantity of urine had (according to the chart) varied between 500 c.c. and 1,300 c.c.; on November 12 it was charted as 300 c.c.; on November 13 as 250 c.c.; on November 14 as nothing at all; but on the last day of his life (November 15) the patient was stated to have twice passed a very little urine into his bed.

NECROPSY AND MICROSCOPICAL EXAMINATION.

The *suprarenal glands* were both transformed into nodulated tumours, containing big veins filled with brownish, friable blood-clot and little cysts filled with black blood-clot. The left gland, weighing 10¼ oz., was considerably larger than the right one, which weighed 7¼ oz. The *left kidney*, weighing 46 oz., was enormously increased in size, owing to its containing masses of brown, friable new growth. The *right kidney*, weighing 8½ oz., likewise contained brown, friable material (new growth) in its medullary portion. In both kidneys there were also small cysts beneath the capsule, containing black clotted blood.

The liver (weight 65 oz.) had a nutmeggy appearance on section, and contained some secondary tumour nodules, and a few cysts filled with black blood-clot and serum. The spleen (weight 9¾ oz.) was firm and of the "cardiac" type, as in cases of chronic passive congestion. In regard to the other abdominal viscera nothing special was noted. The abdominal and other lymphatic glands were not enlarged.

The inferior vena cava was blocked with ante-mortem clot, having a brownish, friable appearance, throughout its whole length; the clot involved both the iliac veins below and extended upwards to the right side of the heart, terminating by a rounded mass which projected into and partially filled up the right auricle. Both the hepatic veins and both the renal veins were similarly affected. The splenic, portal and mesenteric veins were not thrombosed, neither was the superior vena cava nor any of its tributaries. There was some ascites, and the lower extremities were very œdematous, but there was no œdema of the face. Excepting in regard to the thrombus (just mentioned) nothing special was observed in the heart; which (including the intra-cardiac portion of the thrombus) weighed 12¼ oz. There was no pericardial effusion. No marked atheromatous change in the aorta or large arteries was noted. The lungs were somewhat emphysematous and contained a great number of soft, brownish nodules of new growth, about the size of hazel nuts. There was some pleural effusion on the right side, but none on the left.

By *microscopical examination* tumour material taken from both suprarenal glands, from the left kidney, and from the lungs, showed the structure of a papilliferous adrenal carcinoma. The large epithelial-like cells were arranged on a framework of delicate fibrous strands containing the blood-vessels, and these connective tissues strands, with the tumour cells covering them, in many parts of the sections formed

cysts (i.e., papilliferous cysts) with (intracystic) cauliflower-like, delicate branching papillary ingrowths from the cyst walls. This corresponds to the structure of some malignant hypernephromata,[1] and doubtless the primary growth in the present case was suprarenal. There was much fibrous tissue connected with the tumour: a good deal of the tumour substance was necrotic. The thrombus in the inferior vena cava (microscopically examined where it entered the heart) was found to be infiltrated with the same kind of new growth. Renal tissue from the right kidney showed some chronic interstitial nephritis, and the liver showed the centro-acinous necrotic changes of extreme chronic passive congestion.

Summary and Remarks.

In short, the case was that of a man, aged 49, whose suprarenal glands and kidneys on both sides of the body were infiltrated with a form of adrenal carcinoma or malignant hypernephroma. There were likewise secondary nodules of the new growth in the liver and lungs. Venous thrombosis, commencing perhaps in the veins of the greatly diseased and enormously enlarged left kidney, involved both renal veins and both hepatic veins and the whole of the inferior vena cava, the clot extending continuously from the iliac veins below to the heart above. The upper end of the thrombus projected into, and partially occluded, the right auricle. The blood-clot, examined microscopically close to the heart, was found to be secondarily infiltrated by the malignant tumour. The patient died with uræmia and almost complete anuria.

It is remarkable that the patient lived so long as he did, and that his urine, in spite of the involvement of his kidneys, was passed in fair quantity (and contained very little albumin), till a few days before his death. The complete venous thrombosis of his better (the right) kidney doubtless brought about the final, practically complete, suppression of urine.

As in other cases of thrombotic obstruction of the inferior vena cava, the enlargement of the superficial veins in front of the abdomen was a striking feature during life.[2]

[1] In some benign tumours papilliferous cysts form also a marked feature, notably in the minute "papillary adenomata" of kidneys, especially of contracted kidneys. Cf. F. P. Weber, *Trans. Path. Soc. Lond.*, 1898, xlix, p. 176, and 1899, l, p. 179, and the references quoted by him in those articles.

[2] Cf. F. P. Weber, "Ueber die traumatische Thrombose der Vena cava inferior," *Muench. med. Wochenschr.*, 1913, lx, p. 1434; also *Trans. of the Life Assur. Med. Officers' Assoc.*, Lond., 1913, p. 254.

In regard to the occurrence of thrombosis of the vena cava inferior, secondary to malignant disease of abdominal viscera, Dr. Herbert French has recently described and figured a very remarkable and instructive case in the *Transactions of the Medical Society of London*,[1] under the following heading: "Carcinoma of the left kidney, with continuous clot extending from the kidney along the left renal vein and up the inferior vena cava into the right auricle, and producing an intracardiac polypoid mass which led to tricuspid stenosis of the ball-and-socket type; the clot being infiltrated with carcinoma of the same type as that in the kidney itself." French's patient was a man, aged 45, whose chief symptoms were attacks of abdominal pain, with alternating diarrhœa and constipation, associated with pyrexia; the liver and spleen were enlarged, but there was no œdema of the legs. Dr. French states that various diagnoses were favoured during life, but the correct one was only arrived at in the post-mortem room. Likewise in the same connexion, Mr. S. G. Shattock had referred me to "A case of carcinoma of the testis in a young man, with metastatic deposits lying free in the heart and in the inferior vena cava," described by A. A. Kanthack and T. S. P. Strangeways, in the *Transactions of the Pathological Society of London* for 1897.[2]

The suprarenal glands, amongst the various organs of the body, exhibit a noteworthy peculiarity, to which my attention has been drawn by Mr. S. G. Shattock—namely, that they are fairly equally liable to tuberculosis and malignant disease. In both diseases the affection is not rarely bilateral. It is notorious that suprarenal tuberculosis is frequently bilateral, and indeed both adrenal glands may show extensive caseous changes without there being much or any evidence of tuberculosis in other organs of the body. Benignant adenomata of the adrenal cortex are not rarely bilateral, as in a case which I described in 1906.[3] W. Roger Williams, in his book on "The Natural History of Cancer" (London, 1908, p. 301), refers to bilateral tumours of the adrenal glands in the following words: "In infants and adults bilateral forms of malignant adrenal tumours are occasionally seen; Rolleston and Marks, in twenty-six tabulated cases at all ages, met with two instances of this kind. A case which conclusively proves the bilateral origin of the

[1] *Trans. Med. Soc. Lond.*, 1912, xxxv, p. 243.

[2] *Trans. Path. Soc. Lond.*, 1897, xlviii, p. 139.

[3] F. P. Weber, "Symmetrical Adenomata of the Suprarenal Glands, and Extreme Sclerosis of the Aorta," *Trans. Path. Soc. Lond.*, 1906, lvii, p. 338.

disease had been recorded by Breton and Looten. The patient, a woman, aged 57, having suffered for some time from symptoms of adrenal insufficiency, with a painful intra-abdominal tumour, died suddenly. At the necropsy, each adrenal was enormously enlarged by small round-celled sarcomatous growth, which had secondarily invaded, and caused enlargement of, each corresponding kidney, and the adjacent mesenteric glands. No other organs were involved. Similar cases have been reported by Fränkel, Woolley, Suchardt, and others. Adenomata of both adrenals have also been met with (Warthin)."

In conclusion, I must thank Dr. Hans Schmidt and Mr. S. G. Shattock for much help in regard to the pathological investigation of the present case.

Latent Addison's Disease revealed by Death from Pneumonia and Empyema.

By F. Parkes Weber, M.D.

The patient, M. K., aged 35, a Dane, was admitted to hospital on November 10, 1914, with the physical signs of pneumonia of the lower lobe of the right lung, and crepitation (without dullness) at the base of the left lung. His sputum had a "rusty" colour and contained capsulated diplococci. He was very obese and had "striæ atrophicæ" (analogous to the "lineæ albicantes" of women after pregnancy) over the abdomen, as often seen in exceedingly corpulent individuals. These he remembered to have been present for the last twelve or thirteen years. His legs were œdematous and there were purple patches of cutaneous ecchymosis about his ankles and on his feet. He had arrived in London about ten days previously on his way from Buenos Ayres, where he had been ill for some time. In September, 1914, in Buenos Ayres, he had had, according to the account, a transient attack of right hemiplegia. The doctor in Buenos Ayres had said that "his heart was very weak" and had advised his return to Europe. In London he had become acutely ill with orthopnœa and bloody expectoration. During the first four days of his illness in London his temperature was said to have been 102° F., 103° F., and later on 98° to 100° F.

There was a history of malaria in North Argentina about two years ago. He was said to have had tuberculous pleurisy at about the age of 18, and jaundice and kidney disease during childhood. There was no history of alcoholism or venereal disease. It should be stated that on admission the patient had a peculiar yellow tint noticeable in the skin of his face, though his conjunctivæ were not jaundiced.

During the first week under treatment in hospital the patient's condition seemed not to get worse, though there were objective and subjective signs of great general debility. His temperature was nearly always between 98° F. and 99·5° F.; his respiration was at first 40 to 44 in the minute, but afterwards fell to 32 to 34; the pulse was at first 80 to 84, but later on mostly 100 to 108. The urine was of specific gravity 1012 (November 11), averaging 1,200 to 1,800 c.c. in the twenty-four hours, and yielding a "thick cloud" of albumin; a few granular tube casts and uric acid crystals were found in it by microscopical

examination. The signs of pneumonia at the lower lobe of the right lung persisted, but more crepitation than previously came to be heard, as if a certain amount of "resolution" were taking place. Some petechial or ecchymotic patches about the ankles ulcerated. The patient complained a good deal of abdominal pains, and said he had suffered from abdominal pains in Buenos Ayres as well as on the ship during the voyage to England. In regard to treatment, strychnine, digalen and a little alcohol were employed, and xeroform powder was used for the feet. The abdominal pains and discomfort were much relieved by hot hip-baths.

Later on the temperature sometimes reached 100° F., or slightly higher. The brachial systolic blood-pressure was 155 to 170 mm. Hg. The superficial ulceration of the ecchymotic patches (superficial cutaneous gangrene) about the ankles, and some superficial ulceration about the sacrum, required much attention. The sputum was of the "prune juice" type, with a brownish tinge. On November 25 I tapped the right pleura and removed a little fluid, which was not quite clear and which by microscopical examination was found to contain a considerable number of pus cells; the specific gravity of this pleural effusion was 1010, and it "went solid" with albumin on boiling. On November 28 I again tapped the right pleura and withdrew 1,300 c.c. of a fluid somewhat turbid with pus cells. On December 2, when I tapped the right pleura for the third time, I found the pleuritic effusion (about 1,500 c.c. removed) distinctly purulent, foul-smelling, and swarming with cocci. There was therefore a definite empyema, and on the following day my surgical colleague, Dr. zum Busch, kindly operated (under local anæsthesia), resecting a portion of a rib. The patient, however, grew gradually feebler, and died on the night of December 6.

In regard to the *necropsy and microscopical examination*, I am greatly indebted to Dr. Hans Schmidt. The heart (weight 25 oz) was much hypertrophied, especially the wall of the left ventricle; there was no valvular disease or ante-mortem thrombus. There were only slight atheromatous changes in the thoracic and abdominal aorta. A specimen of blood serum from the post-mortem heart's blood gave a definite negative Wassermann's reaction for syphilis. The right lung (side of the empyema) was collapsed and there was an abscess or gangrenous patch in the lower lobe. In the left lung there was hypostatic congestion, and a microscopic section from the lower lobe (where crepitation had always been heard during life) showed the presence of

chronic catarrhal changes. There was no ascites. The liver (weighing 106 oz.) was greatly enlarged, and had a fatty, pale yellow appearance. The upper surface was fixed by a few adhesions to the diaphragm. Microscopic examination showed an extreme degree of fatty change (fatty infiltration of the hepatic cells, doubtless associated with a degenerative condition of the cells themselves). The spleen (weight 6½ oz.) was rather soft and the capsule was attached by some perisplenitic adhesions; a few pale patches observed under the capsule were shown by microscopic examination to be small infarcts. The pancreas appeared normal. The kidneys (weighing together only 8¼ oz.) were rather small, and macroscopically and microscopically showed typical chronic interstitial nephritis. In regard to the other organs—the thyroid gland appeared normal; the testes were small for the patient's size, the weight of these organs (including epididymis) from both sides, together, being only ¾ oz.

Both *suprarenal glands* were enlarged (the left one being about the size of a hen's egg, the right one as large as a goose's egg) and transformed into soft, pale, friable masses, evidently necrotic caseous material, including the whole of the medullary substance, covered only by a thin layer of living cortical substance. Further microscopical examination confirmed the statement that these suprarenal glands consisted of a mass of necrotic caseous material covered by a thin layer of living cortical substance; in this non-necrosed layer were scattered foci of inflammatory small cell infiltration; no tuberculous giant cell systems were found.

Remarks.

It is noteworthy that the post-mortem examination showed no tuberculous changes except in the suprarenal glands, and even in them the tuberculous nature of the caseous change was not definitely proved by microscopic examination.

In the present case the old chronic interstitial nephritis with cardiac hypertrophy evidently to some extent counteracted the tendency towards very low blood-pressure, which is characteristic of typical Addison's disease of the suprarenal glands. On the other hand, the patient's objective and subjective state of debility was characteristic, as well as doubtless the fragility of his skin and the imperfect general reaction of his organism towards infectious disease.

I believe, in this connexion, that prolonged convalescence or a

condition of unusual asthenia following ordinary attacks of infectious diseases sometimes indicates the presence of latent disease of the suprarenal capsules (latent Addison's disease). In December, 1899, I saw a young man, aged 24, suffering from a pyrexial attack, which was regarded as influenza. The illness was accompanied by some pleurisy at the left base, and was followed by remarkable debility. There were several medical consultations about him, and he gradually apparently recovered. Later on, in 1901, when shooting in India, he was attacked by malaria, probably not a particularly severe attack, and was sent up to the hill-station of Darjeeling. There his condition was one of great asthenia without fever, and he had syncopal attacks, in one of which he died. Some subcutaneous pigmentation was observed in India, though none had been noted during the illness in England. A most careful post-mortem examination was made, and in a report sent over to a life assurance office in England it was stated that the suprarenal glands contained yellow caseous material and calcareous plates, and were matted together across the aorta. It seems to me that suprarenal disease (latent Addison's disease) was probably the true explanation of the asthenic condition connected with the previous illness in England.

Medical Section.

April 27, 1915.

Sir William Osler, Bt., F.R.S., and (later) Dr. A. E. Garrod, F.R.S., President-elect of the Section, in the Chair.

A Case resembling Acute Lymphatic Leukæmia, ending in Complete Recovery.

By Arthur J. Hall, M.D.

I have waited some time before bringing forward this case in order to follow it up for a sufficiently long time. It is now more than four years since the acute symptoms disappeared; the patient meanwhile has been in full and vigorous health, has become a father, is actively occupied in his work, and enjoys strenuous outdoor sports.

History: On March 20, 1911, Mr. X, aged 23, consulted his medical attendant on account of swelling of the glands on the right side of the neck. He stated that he had noticed this coming on for a few days previously, but as it had given him no pain he did not seek advice earlier. During the next few days the swelling gradually increased and became slightly painful. He also felt shivery and out of sorts towards the late afternoons, so that he went to bed early. During the last few nights he has been hot and sweated. At this time he did not complain of any sore throat, nor could any cause be found for the glandular enlargement. Slight albuminuria was present, otherwise nothing abnormal.

On March 26 I was asked to see him. There was a large mass of glands in the right neck, extending from the clavicle to the edge of the jaw. Individual glands could be distinctly felt, fairly soft but not

discrete. They were painless and not tender on palpation. Besides this mass solitary enlarged glands could be felt in the right neck, the left neck, both axillæ and both groins. They were all firm and painless. The spleen was felt about ¾ in. below the costal margin. He did not look anæmic. There was no sore throat; his teeth were in good condition. There were no hæmorrhages.

On March 27 a blood count showed: Hæmoglobin, 96 per cent. (Haldane); red blood cells, 5,400,000; leucocytes, 32,000; large lymphocytes, 96 per cent.; polymorphonuclears, 4 per cent. No eosinophiles seen; no evident changes in red blood cells. He had some pain in the glandular mass. Temperature 101° F. in the evening.

A provisional diagnosis of acute lymphatic leukæmia was made, and he was put on to liquor arsenicalis.

On March 29 the glands were smaller and he complained for the first time of sore throat on the right side; otherwise no change.

March 30: Vomiting. A differential count showed: Large lymphocytes, 85·2 per cent.; small lymphocytes, 4·4 per cent.; polymorphonuclears, 10·4 per cent. No eosinophiles in 300 cells; no changes in red blood cells.

March 31: The right tonsil had become more painful and was covered with an exudate. The spleen extended lower in the abdomen. Leucocytes 35,000, with very high percentage of large lymphocytes.

During the next few days, whilst the condition was acute, the case was seen by various leading consultants.

April 5: Glands distinctly smaller, tonsil still swollen; spleen less marked. Leucocytes, 10,800.

April 9: General improvement, but he is easily tired on slight exertion.

It is unnecessary to follow the case in detail. The improvement was steady and continuous week by week. In July the glands were only just palpable and the spleen could hardly be felt. In August he was able to go shooting. From that time to the present he has remained in excellent health.

The condition of the blood has been naturally of considerable interest, and the details of its findings are appended:—

TABLE OF BLOOD EXAMINATIONS.

Date	Hæmoglobin	Red blood corpuscles	Colour index	White blood cells	Mononuclears	Polymorphonuclears	Eosinophiles	Mast cells
1911.	Per cent.				Per cent.	Per cent.	Per cent.	Per cent.
March 28	96	5,160,000	0·94	32,000	90·0	10·0	0·0	0·0
,, 30	—	—	—	—	89·6	10·4	0·0	0·0
,, 31	—	—	—	35,000	—	—	—	—
April 5	80	5,060,000	—	10,800	82·4	16·4	0·2	0·0
,, 9	74	4,300,000	—	6,100	78·2	20·0	1·3	0·5
,, 13	76	5,150,000	—	7,400	68·8	29·2	2·0	—
,, 18	74	4,610,000	—	7,800	77·5	18·0	2·0	1·0
,, 23	76	5,000,000	—	5,800	77·4	20·3	1·8	0·5
,, 27	—	—	—	—	71·0	20·5	7·0	1·5
,, 28	82	5,170,000	—	4,450	73·5	24·75	2·75	—
,, 29	82	5,180,000	—	5,600	—	—	—	—
,, 30	81	5,270,000	—	6,900	—	—	—	—
May 1	81	5,080,000	—	6,400	67·6	27·3	3·0	—
,, 2	81	4,880,000	—	5,600	68·0	27·0	5·0	—
,, 3	80	4,840,000	—	6,200	65·6	27·6	4·3	—
,, 4	81	5,210,000	—	8,600	41·2	54·0	3·6	—
,, 5	82	4,970,000	—	6,800	63·5	32·5	3·0	—
,, 6	—	—	—	—	67·0	26·3	5·0	—
,, 7	83	5,150,000	—	6,000	67·0	28·5	4·5	—
,, 10	83	5,250,000	—	6,800	48·2	50·0	1·8	—
,, 13	90	5,400,000	—	5,400	48·0	48·0	4·0	—
,, 18	—	—	—	—	54·0	45·0	1·0	—
,, 19	93	5,070,000	—	12,800	44·0	54·0	2 0	—
,, 21	95	5,680,000	—	9,800	46·5	50·5	3·0	—
,, 25	88	5,450,000	—	8,400	44·4	50·6	2·0	3·0
,, 28	92	6,000,000	—	7,600	55·2	42·2	2·6	—
June 11	96	—	—	5,600	50·6	46·2	1·6	1·6
August 13	—	—	—	—	53·0	47·0	—	—
September 17	96	5,000,000	—	5,600	50·0	50·0	—	—
October 20	100	5,000,000	—	6,500	50·5	49·5	—	—
November 6	—	—	—	7,000	51·0	49·0	—	—
December 21	100	—	—	6,250	34·0	65 0	1·0	—
1912.								
January 26	—	—	—	8,400	28·0	72·0	—	—
April 1	—	—	—	10,000	35·5	63·6	0·9	—
,, 29	100	—	—	9,000	38·5	60·0	1·0	—
June 25	95	—	—	8,300	50·5	47·5	2·0	—
1913.								
January ...	100	—	—	9,600	47·0	51·0	1·0	—
1914.								
June	—	—	—	9,000	46·6	52·0	0·5	—

REMARKS.

Acute lymphæmia is a disease about which very little is known. Possibly it is not so rare as has hitherto been supposed. During the last few months I have seen four typical cases in which the true nature of the disease was not suspected until the blood was examined. One of

these was thought to be acute rheumatism, another acute phthisis, and two others pernicious anæmia. The reason for this is the absence of any marked enlargement of the spleen or lymphatic glands, suggesting the true diagnosis. Hitherto, however, no case of complete recovery after acute lymphæmia has been observed. It is therefore necessary to be very cautious before assuming that this case belongs to the same category.

In the absence of all knowledge as to the causal agent and of the possibility of demonstrating its presence in the tissues, the diagnosis of acute leukæmia must at present rest upon the symptoms and signs found. Judged by that standard, this case in its acute stage fulfilled sufficient of the requirements to make it distinctly suspicious. The general lymph gland enlargement, with its chief prominence in the right neck, the enlargement of the spleen, the onset of a septic tonsillitis, the presence of a distinct leucocytosis with 90 per cent. of large lymphocytes, and a slight pyrexia—this is a combination of symptoms which almost qualifies for lymphæmia. It is true that the patient did not seem sufficiently ill to suggest so serious a condition, and there was a striking absence of marked anæmia. In all the cases of acute lymphæmia I have seen profound anæmia has been a striking clinical feature—but, after all, this is a quantitative rather than a qualitative difference.

This patient was undoubtedly ill, he was easily tired on the least exertion, so that he wanted to lie down, and although when first seen the red cells were normal in number and the hæmoglobin practically so, yet during the next few weeks there was on repeated careful examination a distinctly lowered percentage of hæmoglobin, which did not return to normal until three or four months later. The red cells, however, kept up to their numbers fairly well throughout. There was also a striking absence of any tendency to hæmorrhages, either into the fundi or elsewhere.

It would be illogical to assume that acute lymphæmia can only exist when it proves fatal, at any rate in the present state of our knowledge. At the same time, looking back upon the case as a whole, my own feeling is that it should not be included under that category. Exactly where to place it, however, I do not know.[1] At any rate, that such

[1] *Note.*—In the discussion which follows it will be seen that Dr. F. Parkes Weber kindly calls attention to a paper published by R. C. Cabot on "The Lymphocytosis of Infection" (*Amer. Journ. Med. Sci.*, 1913, cxlv, p. 335), which deals with cases of an exactly similar type to the one here recorded. I have also found a paper by Marchand, "Ueber ungewöhnlich

cases exist should be remembered as a possible cause of error, and as a warning not to arrive at too hasty a diagnosis and prognosis in atypical cases with glandular and splenic enlargement, and a blood in which nine-tenths of its leucocytic components are large lymphocytes. It is an interesting fact that even after four years the lymphocytic percentage in this case has only once been found to be at the usual figure. In the last count made it was nearly 50 per cent.

As regards treatment, the patient was put on liquor arsenicalis from the first, which was badly tolerated, and for which cacodylate of soda, ¾ gr. t.d.s. given hypodermically, was substituted later. During the early stages rectal lavage with repeated small doses of calomel and soda bicarb. was tried.

Notes on some Cases of Myelogenic Leukæmia.

By Arthur J. Hall, M.D.

Case I: Chronic Myelæmia treated by X-rays; Useful Life for nearly Four Years.

This case, as regards general symptoms and signs, presented no unusual features; it is therefore unnecessary to detail the history at length. Briefly, the definite symptoms began about January, 1907—great weakness, loss of flesh, abdominal pain. These got steadily worse, and he was a complete invalid and could hardly walk.

He came under observation in September, 1907, when he showed symptoms of severe leukæmia. Spleen very large, leucocytes 200,000, hæmorrhages into conjunctivæ, epistaxis, mental symptoms, extreme dyspnœa on least exertion, disks blurred, retinal swelling. He was

starke Lymphocytosis im Anschluss an Infektionen" (*Deutsch. Arch. f. klin. Med.*, 1913, cx, p. 359), which goes more fully into the subject and records several similar cases. After reading these papers it is clear that my case belongs to the same group as those referred to by Cabot and Marchand, and that the question of any relationship to acute leukæmia is sufficiently remote to make any suggestion of their common origin more likely to cause confusion than to advance knowledge. When my case was running its course in 1911 these papers had not yet appeared, and although I find now that occasional similar cases had been recorded in the literature, I did not discover them at the time, nor could I then find a pigeon-hole in which to place my case. Both Cabot and Marchand allude to the difficulty of diagnosing these mild cases from early acute leukæmia until their rapid recovery makes it obvious, and therefore I gather that others have fallen into the same pit in times past.

admitted to a nursing home and X-rays were given daily by Dr. Rupert Hallam. His long progress may be divided for descriptive purposes into five fairly well marked periods, extending from the beginning of treatment in September, 1907, to his death in September, 1911.

Period A (September 13, 1907, to October 7, 1907—about four weeks).—Rest in bed; X-rays daily (ten minutes over spleen and long

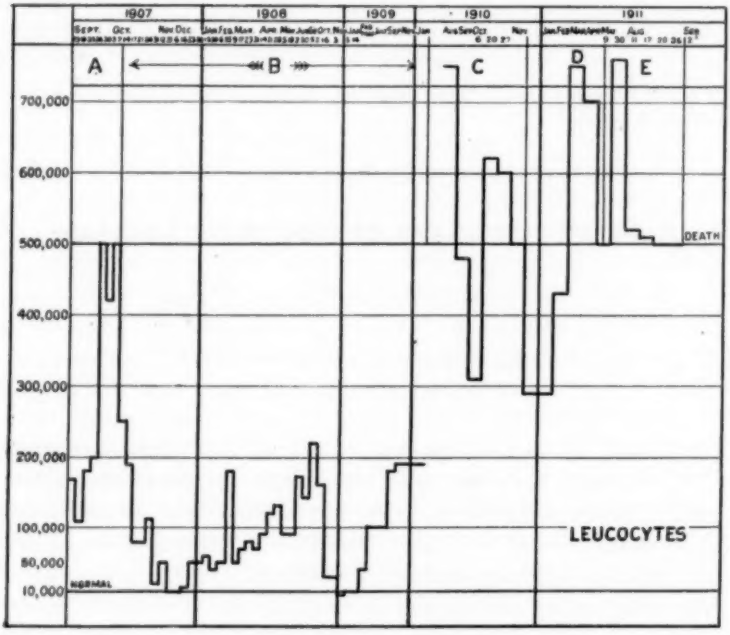


FIG. 1.

Case I: Chronic myelæmia.—Chart of total leucocytes per cubic millimetre extending over four years. The letters A, B, C, D, E, refer to distinct clinical periods during the man's illness. Period B extends from October, 1907, to summer, 1910. The thinner short vertical lines mark the divisions of these periods.

bones). No improvement evident. Leucocytes increased to 500,000, marked increase of myelocytes, red blood cells decreased.

Period B (October 7, 1907, to summer, 1910—thirty-three months). —X-rays continued; liquor arsenicalis begun and pushed as far as tolerated (30 minims daily). Rapid improvement in every way. Spleen

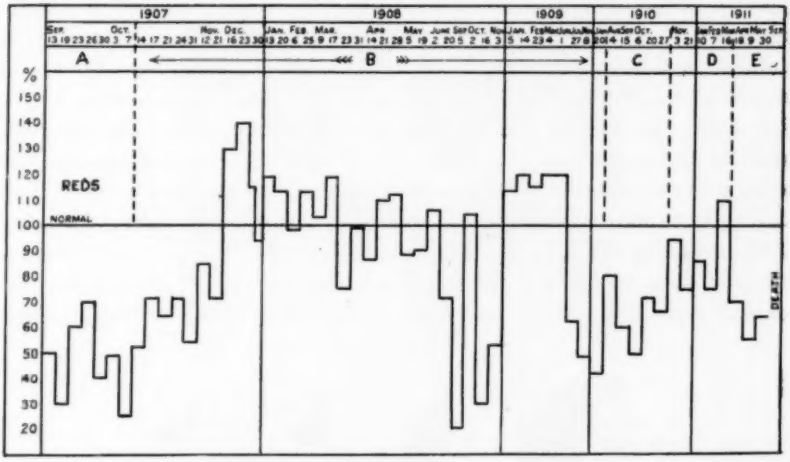


FIG. 2.

Case I: Chronic myelæmia.—Chart of erythrocytes (as percentage) from October, 1907, to September, 1911. Periods A, B, C, D, E, as in fig. 1. separated by interrupted vertical lines.

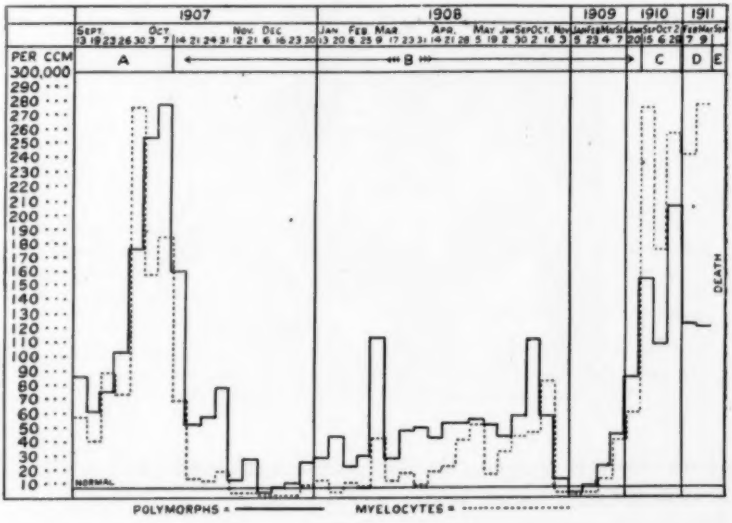


FIG. 3.

Case I: Chronic myelæmia.—Chart of total numbers of polymorphonuclears and myelocytes per cubic millimetre. Lettering as in previous figures.

greatly reduced, leucocytes down to 10,000, reds increased to supernormal. Feels extremely well; able to begin full work as electrician in colliery in November, 1907. During the whole of this period he was in excellent health except for occasional slight breakdowns lasting only a few days. He was at full work nearly the whole time. During 1908 the leucocytes gradually increased, reaching 200,000 in September, but after a slight increase in X-ray treatment at the end of the year they fell again to normal in January, 1909. The spleen also enlarged during 1908, reaching the maximum in September, 1908. He was out of sorts for a week or two at this time and had to be off work. During the rest of this year the spleen again diminished very much. During 1909

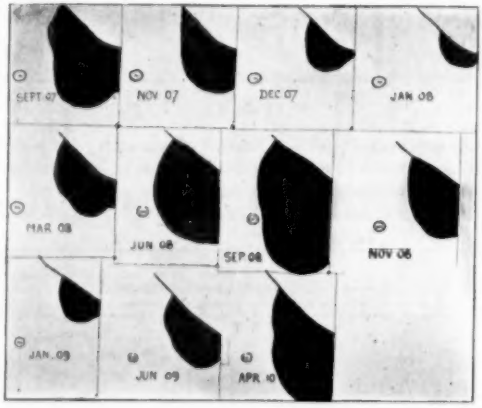


FIG. 4.

Case I Chronic myelæmia.—Tracings of spleen at various dates during periods A and B. Note the rapid diminution during the first four months of treatment by X-rays, followed by gradual enlargement in autumn, 1908, and second diminution in winter, 1909, after increased treatment. April, 1910, represents nearly the end of period B.

Note.—These are actual tracings of the abdomen. In each tracing the dot at the right-hand bottom corner corresponds with the anterior superior spine of the ilium. The position of the umbilicus is indicated in each.

there was little to record. He kept in excellent health and worked full time until the summer of 1910.

Period C (August, 1910, to November, 1910 — four months).— A severe exacerbation; all the original symptoms. Extreme weakness and exhaustion spleen again large, leucocytes up to 750,000, myelocytes

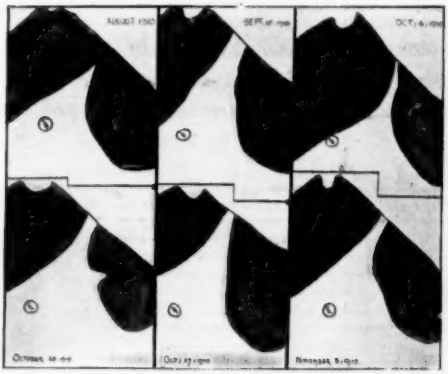


FIG. 5.

Case I: Chronic myelæmia. — Tracings of abdomen during second exacerbation (period C). Definite enlargement of liver is now first noticeable, also some ascites.

Note.—The particulars of markings in these are identical with those mentioned under fig. 4.

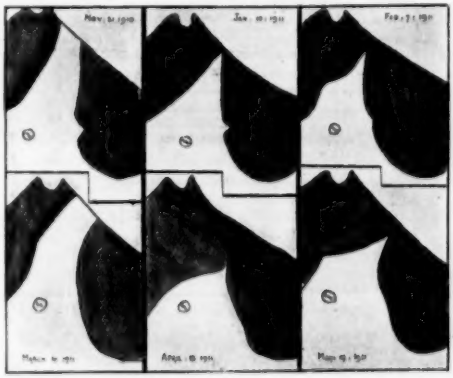


FIG. 6.

Case I: Chronic myelæmia.—Tracings of abdomen during periods D and E.

Note.—Excellent subjective health and full working capacity in spite of enlargement of liver and spleen, as seen in tracings of January, February and March, 1911. Markings as in figs. 4 and 5.

relatively much increased, red blood cells subnormal, liver much enlarged, ascites, œdema, choked disks, vision impaired. X-rays had been reduced to about one application per month during this year. In October, 1910, they were begun again vigorously by Dr. Nutt at the Royal Hospital, and general improvement followed quickly.

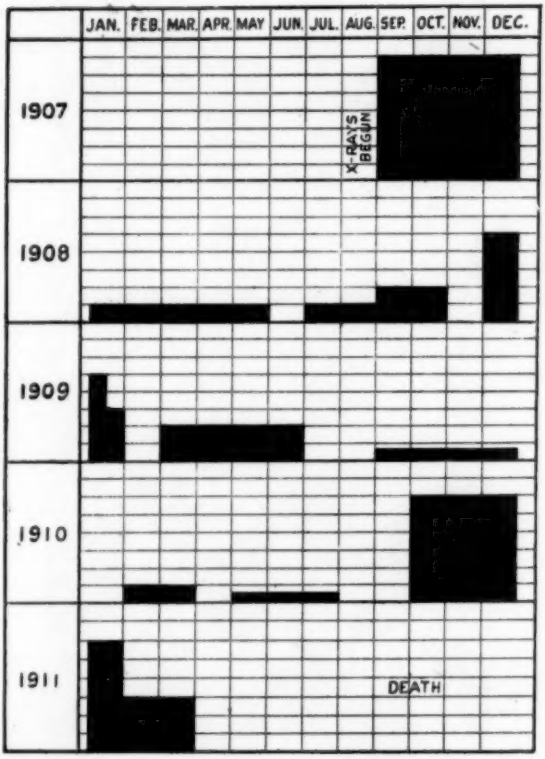


FIG. 7.

Case I: Chronic myelæmia.—Chart of periods of X-ray treatment. Horizontal intervals represent "days per week" on which exposures were made. Thus, September, 1907, seven days per week; May, 1908, one day per week.

Period D (December, 1910, to April, 1911—five months).—He was now subjectively quite well again and able to do full work, and after work travel by train to Sheffield (about 18 miles) for X-ray treatment

every evening for some weeks. The sight had completely recovered; the disks cleared up; spleen and liver were still large; leucocytes still at a very high level, the lowest count being 300,000; red blood cells rather increased, at times above normal; myelocytes always relatively high.

Period E (May, 1911, to September, 1911—four months).—Final exacerbation: Progressive weakness, emaciation; liver much enlarged; ascites, paracentesis, 9 pints of fluid removed; severe recurrent epistaxis; prolonged bleeding from prick made for blood examination.

Death, September 3, 1911.

The features to which I would call attention in this case are: (1) The complete subjective euphoria following treatment during two severe exacerbations. (2) The very long period, nearly three years, during which the first improvement was maintained—November, 1907, to Summer, 1910. (3) The much shorter interval of improvement after the second exacerbation, but its completeness as regards physical capacity for work, although on this occasion the leucocytosis was greater than during the exacerbation period of 1907. (4) The first appearance of definite liver enlargement at the second exacerbation (August, 1910) and its permanency, though diminished in size from that time onwards.

CASE II: CHRONIC MYELÆMIA; X-RAYS; COMPLETE RECOVERY OF GENERAL HEALTH AND STRENGTH; ACUTE SYMPTOMS ENDING FATALLY SIX MONTHS LATER.

C. B., male, aged 41. He began to feel ill in February, 1907, being feverish at night and delirious. In April a doctor said there was some large substance in his abdomen. A year later, in May, 1908, he himself felt his enlarged spleen, and was very weak and ill. In September, 1908, the spleen was extremely large, and he had 300,000 leucocytes per cubic millimetre. He was put on to liq. arsenicalis 15 minims daily, gradually increased, and had X-ray applications daily for ten minutes over the spleen and hip or knee. In December he said, "Life is worth living now." He did full work as a teacher, and whilst the leucocytes had dropped to under 10,000, the spleen was only just palpable under the costal margin.

He continued in the same condition until March 27, 1909, when he became feverish, felt faint, and vomited frequently. The spleen enlarged rapidly; leucocytes 164,000. Pulmonary symptoms suggesting broncho-pneumonia rapidly developed, and he died on April 3.

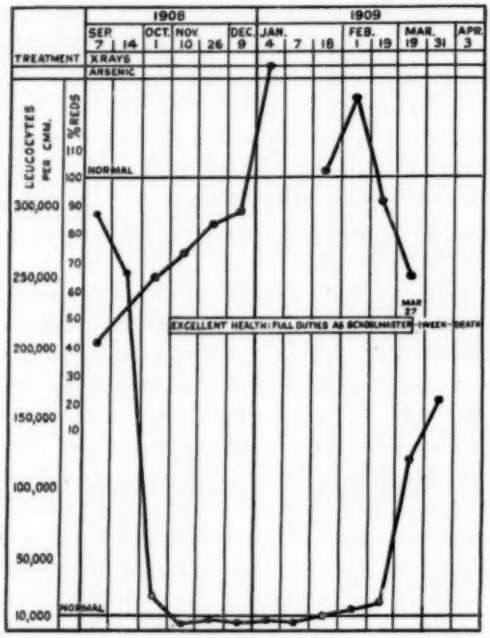


FIG. 8.

Case II: Chronic myelæmia.—Chart of total red blood cells and leucocytes per cubic millimetre whilst under observation and treatment. The ascending line = reds. The falling line = leucocytes.

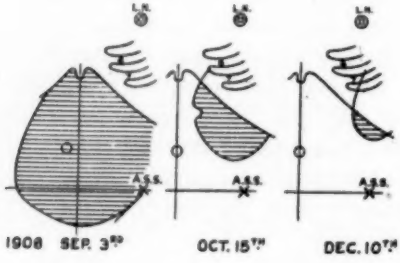


FIG. 9.

Case II: Chronic myelæmia.—Tracings of spleen. Left nipple, umbilicus, and anterior superior spine indicated in each case.

He had X-rays regularly until the end of November, 1907. Arsenic was also given until early in December and was then omitted on account of toxic symptoms. It was again given a few weeks later and continued at intervals until his acute final illness.

Case III: Acute Leukæmia with Extreme Bone Pains.

Male, aged 38, teetotaller and non-smoker. Whilst away on holiday in June, 1914, he began to have severe pains, chiefly in the back and shoulders. There was a history of slipping and straining himself on June 1, so that he had to take to his bed. The pains continued on and off, and about the end of July they became more extensive, severe and continuous. Slight pyrexia, about 99° F. They kept him awake at night.

I was asked to see him on September 9, as a case of subacute rheumatism. The joints were not at all painful, but the long bones, particularly in the arms and about the shins, were chiefly the seat of trouble. Examination proved negative, except that the spleen was just palpable below the ribs, and he was undoubtedly pale. The pallor, I was assured, was no more than it had been all his life, and this was confirmed by his doctor and his relatives. It had, however, a pathological appearance, and a blood film revealed the true nature of the case. Blood count showed: Hæmoglobin, 50 to 60 per cent.; leucocytes, 100,000; red blood cells, not counted; nucleated red blood cells, extremely abundant (11 to every 100 white blood cells); polychromasia; basophilia. Hæmorrhages into retina of right eye.

Personally, I did not have an opportunity of seeing him again, but he steadily got worse and died in a few weeks.

The striking feature of the case from beginning to end was the extreme pains in the trunk and limbs. Nothing gave him any relief except morphia, and his sufferings were, I am informed, very distressing. I have seen cases of chronic leukæmia in whom pains have been present from time to time in the limbs, and I have also seen this in cases of acute leukæmia, but they have never, in my experience, dominated the picture as they did in this case.

CASE IV: CHRONIC MYELÆMIA IN A WOMAN WITH COMPLETE DISLOCATION OF SPLEEN.

The patient was first seen by a surgeon on account of an abdominal tumour, which, arising in the pelvis, reached upwards above the navel. It was smooth, firm, and rounded, with a completely free edge above. A pelvic tumour was diagnosed and laparotomy performed. It was found to be an enormous spleen completely dislocated, and resting on the floor of the pelvis. Fortunately, no attempt was made at removal. A blood examination followed and showed the true nature of the case, which ran an ordinary course, apparently not aggravated by the surgical interference.

An Instance of Long Quiescence of Leukæmia.

By F. PARKES WEBER, M.D.

THE patient, Mrs. E. P., then aged 63, sought hospital advice in March, 1909,[1] for what she described as "bearing-down pains," "as if she were going to have a child." These pains seemed to come on when she became fatigued from walking, &c. Nothing abnormal was detected by gynæcological examination, but the spleen was found to be greatly enlarged, extending downwards distinctly beyond the anterior superior iliac spine. The liver could not be felt. The inguinal lymphatic glands were moderately enlarged on both sides, the axillary glands (on both sides) could only just be felt, and the cervical glands were apparently not enlarged at all. From March 16 to May 27, 1909, the patient was in the German Hospital for treatment by atoxyl injections and radium applications (Dr. Finzi). The spleen distinctly decreased in size under the treatment, and the lymphatic glands in the groins became scarcely larger than they are in some healthy individuals. On December 6, 1909, the spleen did not quite reach the level of the anterior superior iliac spine.

The table on following page shows the results of blood examinations up to the end of the year 1909.

[1] *See* F. P. Weber, "Case of Chronic Lymphocytic Leukæmia without Anæmia," *Trans. Med. Soc. Lond.*, 1910, xxxiii, p. 364.

The differential count on March 4 was kindly made by Dr. A. E. Boycott. Both eosinophiles and mast cells were found, though none were present amongst the cells counted. The differential counts on the later occasions were kindly made by Dr. Pfister and Dr. Dorner. The red cells appeared normal. No nucleated red cells were ever noted.

I was able to examine the patient again on December 4, 1911, when she complained of rheumatic-like pains in her feet, but looked well. The inguinal glands were moderately enlarged on both sides, and the spleen could be felt extending down to below the anterior superior iliac spine. Blood examination (Dr. G. Dorner): Hæmoglobin, 70 per

	March 4, 1909	March 29, 1909 (before radium treatment)	May 26, 1909 (after radium treatment)	December 6, 1909
Red cells per cubic millimetre of blood	4,100,000	4,176,000	3,800,000	5,740,000
Hæmoglobin per cent. ...	80	—	75	82
White cells per cubic millimetre of blood	57,000	43,200	12,000	47,120
Differential Count of White Cells per cent.				
Lymphocytes	94·0	99·0	84·0	89·4
Neutrophile polymorphonuclears	0·8	0·5	14·0	10·0
Large hyalines	2·4	—	—	—
Intermediates	2·8	—	—	—
Eosinophiles	0·0	0·5	2·0	0·2
Basophiles	0·0	—	—	0·4

cent.; red cells, 5,380,000 to the cubic millimetre of blood; white cells, 95,600. The differential count of white cells gave: Polymorphonuclear neutrophiles, 3·2 per cent.; lymphocytes, 96·6 per cent.; eosinophiles, 0·2 per cent. The erythrocytes appeared normal and no nucleated red cells were seen.

In March 1914, owing to the kindness of Dr. A. E. Garrod and his house physician, Dr. Evans, I was able to examine the patient again whilst she was in St. Bartholomew's Hospital. She looked much the same as she did previously, and complained of a little giddiness. A doctor, who had chanced to notice the enlarged spleen, had sent her to the hospital. The blood count showed: Hæmoglobin, 77 per cent.; red cells, 5,160,000 to the cubic millimetre of blood; white cells, 123,800. A differential count of the white cells showed that 96·5 per cent. were lymphocytes.

Remarks.

Looking back on this case, it appears doubtful whether the symptoms which from time to time induced the patient to seek medical advice had any causal connexion with her chronic lymphocytic leukæmia, although in 1909 a real improvement was perhaps associated with the reduction in the size of the spleen by the radium and atoxyl therapy. In chronic lymphocytic leukæmia extreme enlargement of the spleen as in this case is an unusual feature, and perhaps the absence of anæmia has some prognostic importance. May not this absence of anæmia be taken as showing that the red bone-marrow is not overwhelmed by the leukæmic disease, and that (in spite of the leukæmia) it remains able to maintain its normal erythropoietic activity, which is so essential to the well-being of the whole organism?

It is quite conceivable that cases similar to the present one occur in which the total of white cells is much less than in the present case. The possibility of such a case being one of so-called "Banti's disease," (an example of permanently or temporarily "compensated Banti's disease," without any considerable anæmia) might arise, but I believe that in all such cases of chronic "Banti's disease," as well as in practically all cases of chronic splenomegaly, apart from the leukæmias, the tendency is towards a leucopenic blood count rather than towards any excess in the number of white cells. A condition of chronic splenomegaly combined with any numerical excess of white cells, should always arouse a suspicion of the case being one of chronic leukæmia (though possibly in a more or less quiescent state).

One might think that in the present case the long quiescence of the leukæmia had to do with the patient's advanced age, just as certain diseases, such as diabetes mellitus, tend to assume a more benign form in elderly persons, but this can scarcely be admitted, for various cases of rapidly fatal leukæmia in old persons have come to my knowledge.

The quiescence and long duration of the disease in the present and several other published cases furnish a ray of "prognostic hope" in leukæmia, but one must admit that no undoubted case of any kind of leukæmia has ever been found to recover, with or without any known method of treatment. A patient supposed to have recovered (1908) from acute myelocytic leukæmia under treatment by naphthalene tetrachloride died after all, I hear, three years or so later, from the same disease. Myelocytosis, due to fracture of bones, metastatic

tumours in the bones, or severe infections, may sometimes have been mistaken for myelocytic leukæmia. Cases of supposed recovery from lymphocytic leukæmia have probably been examples of unusual lymphocytosis from infection, similar to the cases recorded by Fritz Marchand[1] and R. C. Cabot.[2] Cabot specially refers to lymphocytosis of that kind, which may be due to streptococcal infections (as in certain cases of wound sepsis, boils, and tonsillar adenitis), being mistaken for lymphocytic leukæmia, with a correspondingly gloomy mistake in the prognosis (as a result of the wrong diagnosis). Possibly also certain congenital syphilitic cases have been occasionally confused with leukæmia.

DISCUSSION.

Professor Sir W. OSLER said he had been much interested in Dr. Hall's first case, the history of which he had carefully followed. When first seen it was certainly very suggestive of acute lymphatic leukæmia. The favouring point in his mind was the fact that the leucocytosis was not extreme. The question was, whether they could diagnose lymphatic leukæmia on the differential count alone without a total increase in the leucocytes. They knew that a leucocytosis, chiefly of the polynuclears, was associated with a great majority of the infections. In a few, particularly whooping-cough, the lymphocytes were chiefly enlarged. The doubtful cases were those with sore throat, adenitis, and marked lymphocytosis; the total count might go to 30,000 or 40,000, a large proportion of which might be lymphocytes. A patient of Dr. Bell's, of New Brighton, Cheshire, had been a source of clinical embarrassment to Dr. Abram, Dr. Buchanan, and himself. Following an attack of enlarged glands with slight enlargement of the spleen there had been a marked lymphocytosis, which was regarded as an acute leukæmia, though the leucocytes never rose above 30,000 per cubic millimetre. A report from one of the clinical laboratories was returned with a diagnosis of acute leukæmia. The patient had four injections of salvarsan and was now quite well. The case would be reported fully and the diagnosis was under discussion.

Dr. GALLOWAY said that he had gained much from listening to Dr. Hall's very interesting paper. Perhaps the most instructive point was the account of the remarkable case of lymphæmia which they had heard. The report was a very satisfactory one, and helped perhaps to lighten the prognosis of a disease of which the prospect was at the present time very bad. Whether such a case of lymphæmia should be considered to be a definite disease entity, or whether it should be regarded as an early or mild case of lymphatic leukæmia, further

[1] "Ueber ungewöhnlich starke Lymphocytose im Anschluss an Infektion," *Deutsch. Arch. f. klin. Med.*, Leipz., 1913, cx, p. 359.

[2] "The Lymphocytosis of Infection," *Amer. Journ. Med. Sci.*, 1913, cxlv, p. 335.

observations in the future would perhaps show. At least it gave reason for considering carefully, and possibly withholding an entirely unfavourable diagnosis in certain doubtful cases with leukæmic characters which occasionally came under observation. It was hardly to be expected that every case of leukæmia should be so severe as those usually under treatment in general hospitals. They had a good deal of experience of early cases of the disease—there might be mild and curable ones. The second point arising from the paper which he wished to discuss was the treatment, especially by means of X-rays. In the early days of X-ray treatment he remembered very well indeed a woman, aged about 27, coming under his care at Charing Cross Hospital, suffering intensely from spleno-medullary leukæmia—a well-marked myelæmic case. The spleen was greatly enlarged; an enormous number of leucocytes were present, with a large proportion of myelocytes. She was pallid, breathless, scarcely able to move, and a fatal termination appeared to be imminent. The question of X-ray treatment had just been mooted in such cases, but so far as he was aware no similar case had been reported as being treated in London. Treatment by means of X-rays was begun by his colleague, Dr. Ironside Bruce. To their great satisfaction marked improvement rapidly occurred, the spleen lessened in size, the number of leucocytes diminished, and the patient rapidly improved in health. She left the hospital, and for a few years after continued under observation and treatment, having repeated courses of X-ray exposures. During the greater part of this time she was in good health, able to conduct her household duties, and she could walk five or six miles a day without any undue fatigue. At the end of this period she rapidly lost health, developed marked anæmia, with excessive leucopenia, and died almost as if she had wasted away. The blood-forming organs seemed to lose the power of producing either red or white cells. For some time before her death care had been taken not to press the X-ray exposures, as by that time experience as to the effect of X-rays had gradually been acquired. Such a remarkable case as this had encouraged the speaker to use X-ray treatment in several similar cases, but unfortunately by no means always with such a good result, and it was especially found that the results seemed to be very uncertain. It was not uncommon to find that when the number of the white blood cells diminished the spleen also decreased in size, but under these circumstances it was not always the case, though perhaps it was the rule, that the patient improved in general condition. On the other hand, some cases of undoubted myelæmia apparently did not respond favourably to treatment. It would be well if in the course of the discussion indications could be stated likely to be improved by X-ray treatment, as well as the characteristics of those cases not likely to improve. Dr. Galloway had already remarked in a previous discussion at the Royal Society of Medicine that it was possible that the use of so powerful an agent in cases of this disease might do harm instead of good. The symptoms might possibly be part reaction of the tissues to the poison of the disease, and more harm than good might come from checking this reaction. It appeared to be the case, however, that the presence of an excessive number

of leucocytes was a grave indication, and improvement seemed to occur when the leucocytes diminished to what might be called a moderate amount. With reference to other methods of treatment, on several occasions he had found the patient derive benefit from the administration of arsenic, either internally or by subcutaneous injections. He had no personal experience of treatment by means of drugs of the naphthol or benzol series, but he might draw attention to a patient at one time under his observation in whom apparent temporary improvement occurred after an attack of acute facial erysipelas. In his experience the treatment of cases of lymphatic leukæmia belonged to a different category from that of cases of myelæmia, and this was especially so as regards X-ray treatment.

Dr. IRONSIDE BRUCE said that with regard to the X-ray treatment of spleno-medullary leukæmia a point of importance was the method employed. He had found that the use of a "filter" of some kind was necessary to prevent burning of the skin, and he was in the habit of employing several layers of felt for this purpose. Such a filter protected the skin, and it was found that the exposures produced the same result as shown by blood counts with the use of the filter as without it. The number of exposures to be given in any particular case should be controlled by the condition of the blood, and he had found that when the white count reached 40,000 it was better to stop for a time. He had had experience of a case in which the white cells, having fallen to about normal, continued to fall until a point far below normal was reached, the red cells also diminishing in number and a fatal result following, whether from the effects of X-ray exposure or a natural termination of the case he was unable to say. There was, in his experience, always a period after commencing treatment during which time the effects of the exposures were not seen in the blood count, so that after stopping a similar period might be expected during which the treatment would continue to produce effects on the blood. Exposures of the spleen only seemed to produce results similar to exposures of the spleen and the ends of long bones, so that he had not continued to include the long bones in the area of exposure. The effect of X-ray exposure was most marvellous, and undoubtedly a period of variable length, during which the patient might return to apparent good health, was to be expected, but the prognosis was not affected in any way, at least so far as his own personal experience was concerned.

Dr. F. PARKES WEBER said he believed that Dr. A. J. Hall's case presenting symptoms of lymphatic leukæmia was really an example of unusual lymphocytosis of infection, similar to the cases described by R. C. Cabot and others. Nobody really knew why the infecting microbes in such cases should give rise to a lymphocytosis instead of the well-known "polymorphonuclear" leucocytosis or a myelocytosis.[1]

[1] The benzol treatment of leukæmia, in Dr. Weber's limited experience, had been disappointing, and he did not know of any specially brilliant results having been obtained by means of salvarsan or neo-salvarsan.

Dr. HALL, in reply, thanked the members for their interesting remarks on various points in the cases. He was rather surprised that no mention had been made by any of the speakers of the benzol treatment of leukæmia. Personally, he had only tried it thoroughly in one case, and the results had been disappointing.